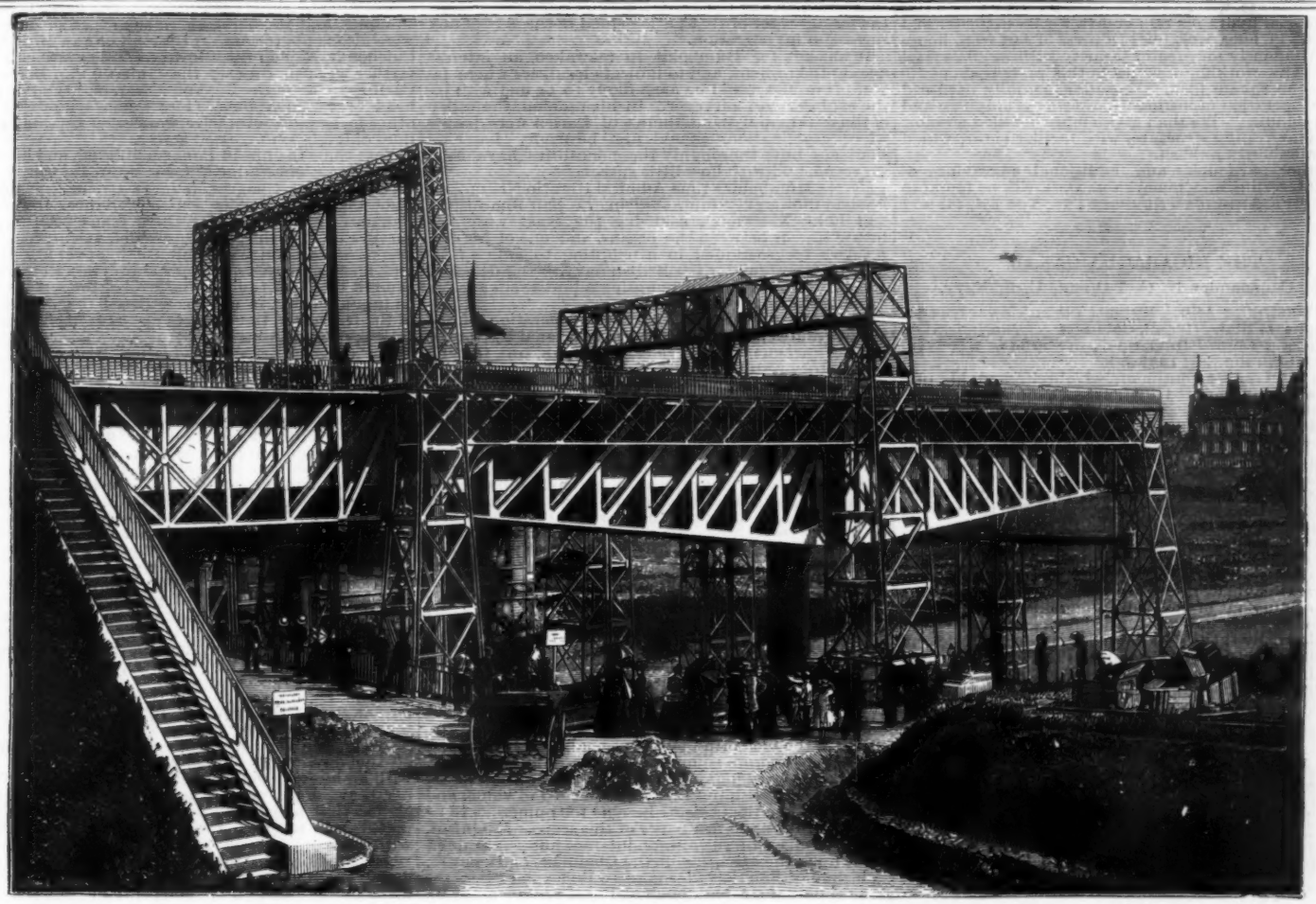
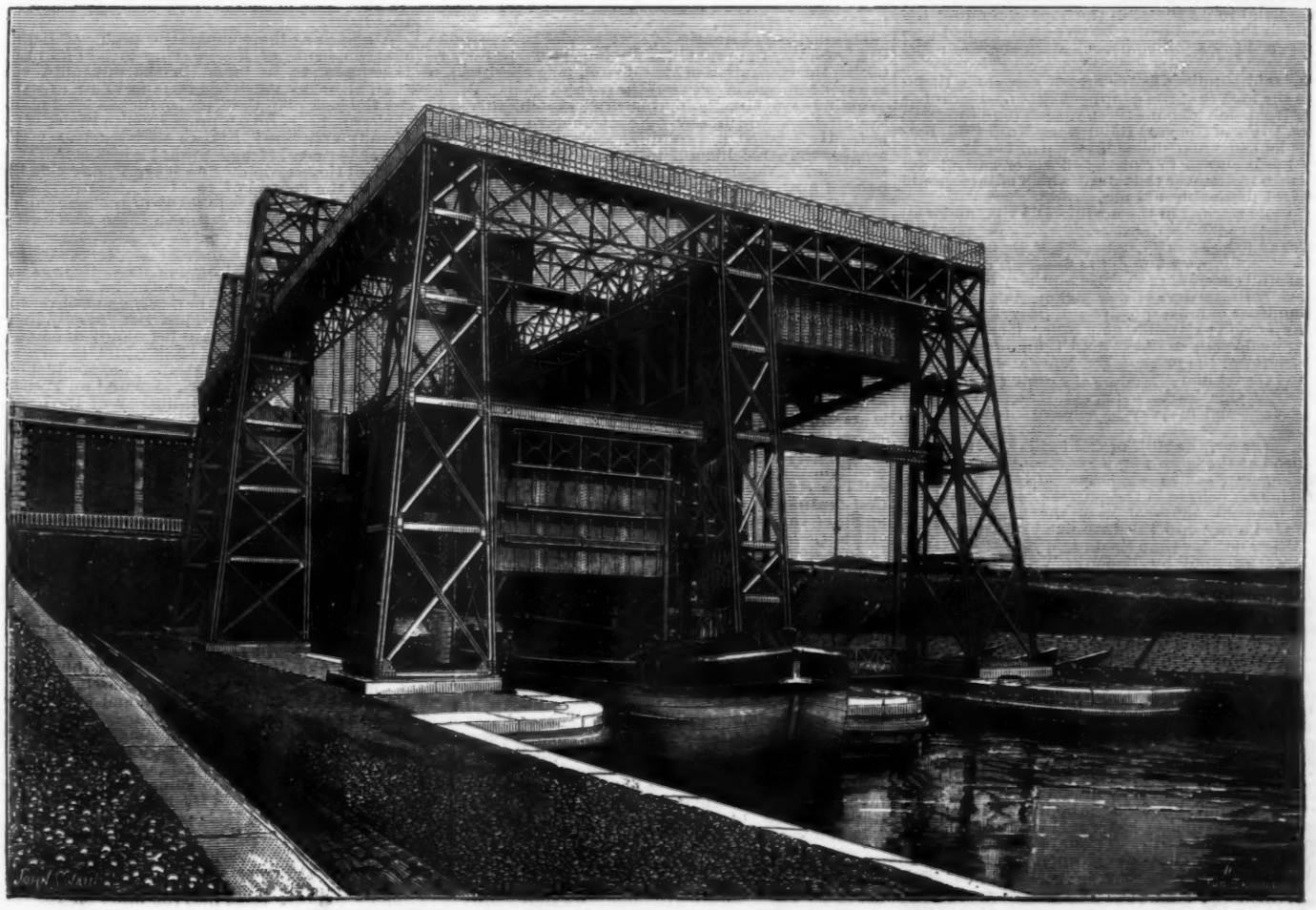
SCIENTIFIC AMERICAN
No. SUPPLEMENT 683

Scientific American Supplement, Vol. XXVII., No. 683.
Scientific American, established 1845.

NEW YORK, FEBRUARY 2, 1889.

Scientific American Supplement, $5 a year.
Scientific American and Supplement, $7 a year.





HYDRAULIC CANAL LIFT, LA LOUVIERE, BELGIUM.

LA LOUVIERE HYDRAULIC CANAL LIFT.

WE give two illustrations from photographs of this, the largest hydraulic canal lift in the world, which has been completed during the last summer. It was constructed for the Belgian government by the Societe Cockerill, of Seraing, from the designs of, and under the superintendence of, Messrs. Clark, Standfield & Clark, of Westminster, the consulting engineers of the government and the patentees of the system. The difference between the levels of the upper and lower canals, that is, the height the boats are raised, is 50 ft. 6¼ in. The lift consists of two pontoons or troughs, each 141 ft. long by 19 ft. broad, with 8 ft. draught of water, and are capable of holding the largest size of barge that navigates on the Belgian broad gauge canal system; such barges are capable of taking 400 tons of coal or other cargo, so that the total weight of the trough, water, and barge is not much under 1,000 tons. This immense weight is supported on the top of a single colossal hydraulic ram of 6 ft. 6¾ in. diameter and 63 ft. 9½ in. long, working in a press of cast iron, hooped continuously for greater security with weldless steel coils. The working pressure in this press is about 470 lb. to the square inch. The time actually occupied in the operation of lifting or lowering is only two and a half minutes. We shall give a more detailed account of this interesting structure, with further views, which we shall shortly publish.

Further particulars and additional engravings of this interesting and remarkable work will be found in SCIENTIFIC AMERICAN SUPPLEMENT, NOS. 334, 447 and 671.

THE NICARAGUA CANAL*

By Commander H. C. TAYLOR, U. S. Navy.

MR. PRESIDENT AND MEMBERS OF THE INSTITUTE:

It appears that, after all, Columbus was not mistaken in sailing westward from Spain in search of a convenient route to the East Indies.

The intervening continent, which checked his progress and disappointed his successors in American exploration, made his avowed attempts to reach the Indies a complete failure, though the memory of this failure was lost and obscured in presence of the glory of a greater success, the undreamed glory of a new world.

It is a pleasing thought that four centuries later, perhaps in the very year 1893, we shall see the wisdom of that valiant spirit justified, and European fleets sailing westward from Spain through an isthmian ship canal as the most convenient route to the far East. The mind dwells with satisfaction upon this justification of the great navigator, and sees with a singular pleasure ships coming out of Cadiz, dropping almost immediately into the soft current of the northeast trade winds, and blown by them smoothly across the Atlantic and Caribbean, between the islands of the West Indies, and quite into the entrance of a Central American ship canal; issuing thence into the same belt of gentle, favoring winds, and carried by them across the broad Pacific to those rich coasts and islands of the far East which were ever such powerful magnets to the earlier navigators. Nor is this pleasing thought any longer an unreasonable one. That ships shall cross the isthmus from sea to sea has been decided. The clamor of commerce and its shipping can no longer be disregarded. Transit across the narrow lands of Central America is certainly to be provided, and the only questions are when, where, and how.

The search for a practicable canal route succeeded to the long, persistent examination which had caused explorers for so many years to penetrate every inlet from Newfoundland to La Plata, in the hope of finding the strait which they confidently believed nature had provided as a means of communication between the oceans.

"Men," said Humboldt, "could not accustom themselves to the idea that the continent extended uninterruptedly from such high northern to such high southern latitudes." From the year (1513) when Nuñez de Balboa first looked upon the wide sweep of the Pacific, a century was occupied in fruitless efforts of gallant and capable men to discover that strait which nature should have placed there—but did not.

The Cabots worked in the north. D'Avila, under secret orders of the Spanish king, scrutinized eagerly the isthmuses and the Spanish Main; while De Solis, under similar instructions, explored the coast of Brazil, and while hopefully ascending the great estuary of La Plata was killed by the natives of that region. Ponce de Leon sailed hundreds of miles northward from Panama on the same errand; Cabrillo and other lieutenants of Cortez groped north and west from Tehuantepec, as far as the vicinity of the present Monterey and San Francisco; and Cortez himself, under the urging of his royal master, King Charles V., of Spain, struggled against much obstacle and disaster to achieve the desired discovery.

When, however, the Gulf of California was found to have a head at the mouth of a great continental river, the intelligent Spanish explorers, already doubtful, could no longer believe in the existence of any communication between the seas, and the "secret of the strait" faded away into the dreamland of legend and fable. Other nations than Spain still hoped. As late as 1607, we are told by Bancroft, Virginia colonists were ordered to seek communication with the South Sea "by ascending some stream which flowed from the northwest," and that it was in ascending the Chickahominy with this end in view that Captain John Smith was captured by the natives; and thus another touch of interest is added to the adventurous record of this man of ordinary name and extraordinary life.

The strait was an idea difficult to surrender. It *ought* to be true, they said. The seas are so close together for 1,000 miles. Commerce between "Cadiz and Cathay" so greatly needs it. It *must* be so. That it should not be was, in the words of the writers of that day, "repugnant to the interest of humanity." The "secret of the strait" *must* be disclosed.

The world moving, like all large bodies, slowly toward conviction, did become at last convinced that nature had not pierced the barrier for our use and comfort, and this conviction once forced upon it, plans for an artificial channel began soon to be suggested. The idea had been touched upon by Balboa, Cortez, and Saavedra, but the first record we have of a practical suggestion is that of the Spaniard Gomara, who urged the idea upon Philip II., in 1551, but the son was not the father; nor were such leaders as Cortez to be found, even had the spirit of Charles V. still animated the actions of the Spanish throne.

From this time forward, the Spanish government seemed disposed rather to smother than to encourage any efforts to connect the oceans. As the old-time Spanish vigor departed, the feeling grew that if any route were found, it would only be snatched from them by some of those daring Drakes and Grenvilles who, roaming the seas at the head of brave and reckless companions, sought every opportunity to insult Spain and plunder its colonies.

A long period now passed, during which no interest was evinced in the canal question. The mystery with which the Spanish government had wished to cover it was complete. If the desire for knowledge came later, the failing vigor of that nation stood in the way of any successful investigation.

It was left for Humboldt to reawaken an interest among the nations, and to indicate localities where favorable results would be most likely to be met with by the explorer and surveyor. In his opinion, the valley of the Atrato and the Isthmus of Darien were points where examinations should first be made. Later we shall see how thoroughly these localities have been surveyed, and with how little success.

Before Humboldt's time Admiral Horatio Nelson had become thoroughly interested in the San Juan River and Lake Nicaragua, as being the great route for future transit between the oceans, and one of his few reverses was sustained in attempting to gain a foothold in Nicaragua for the British navy, being defeated and driven back while attempting a hostile advance or reconnaissance up the San Juan.

In 1825 Nicaragua invited the co-operation of the United States in the construction of a canal by way of Lake Nicaragua and the river San Juan, but with no satisfactory results. In the same year Mexico caused a rough survey to be made of the line *via* the Coatzacoalcos River and the Isthmus of Tehuantepec, resulting in an official report that the "canalization of this isthmus was problematical and gigantic."

Later, in 1828–29, a survey was made under orders from General Bolivar of a route substantially the same as that of the present railway between Aspinwall and Panama. Nothing was effected by this action, except to put an end to the popular error that the mean levels of the oceans on opposite sides of the isthmus differed appreciably. There are still some, I believe, among those who have not given their attention to this subject, who are yet ignorant that differences are caused only by tides, winds, barometric pressures, and other temporary causes, and that the levels of the two seas may be regarded as practically the same.

Many other attempts were made in the ensuing years, and with uniform lack of success. In 1830, the Netherlands, beginning fairly enough, were obliged by the revolution in Belgium and its separation from Holland to give up the project. In 1835, President Jackson appointed Charles Biddle as special agent to promote the idea of an isthmus canal, and to visit the Central American countries for that purpose. Mr. Biddle met with many difficulties, and returned to the United States with nothing accomplished of value, and from this time the projects became too numerous to be even touched upon in a paper of this scope. M. Guizot, under Louis Philippe, urges interoceanic canal questions upon the attention of the Chamber of Deputies. A bishop of San Salvador goes to Rome and urges the importance of a canal upon the Pope. All of no avail; but in 1849, a success is scored, not for a canal, but for the Panama railway.

This method of transit, of vast service to commerce, has, however, by providing an imperfect system, retarded the realization of that dream, long cherished, of a water connection between the oceans.

We must not omit, in this brief sketch, to record the attention which Louis Napoleon gave to the project. He studied it carefully, and at one time thoroughly prepared himself to assume the presidency of the "Canal Napoleon de Nicaragua." His views are best set forth by quoting from his own writing in the magazines of that day, before he became Emperor of the French:

"The geographical position of Constantinople is such as rendered her the queen of the ancient world. Occupying, as she does, the central point between Europe, Asia, and Africa, she would become the *entrepot* of the commerce of all these countries, and obtain over them an immense preponderance; for in politics, as in strategy, a central position always commands the circumference. There exists in the new world a state as admirably situated as Constantinople. We allude to the State of Nicaragua. As Constantinople is the center of the ancient world, so is the town of Leon the center of the new, and if the tongue of land which separates its two lakes from the Pacific ocean were cut through, she would command, by virtue of her central position, the entire coast of North and South America.

"The State of Nicaragua can become, better than Constantinople, the necessary route of the great commerce of the world, and is destined to attain an *extraordinary degree of prosperity and grandeur.*"

Here, at Brito, instead of Leon, will rapidly be created one of the great world centers of industry, an *entrepot* of vast international commerce, a focus of interchange for the products of the globe.

We must now, having glanced briefly at the past history of this great problem, make a rapid review of the work of to-day, and consider the various routes which have been examined during the last few years. Although the boiling down process of precise instrumental surveys has reduced these possible lines of transit to three, Panama, Nicaragua, and Tehuantepec, and although the further process of actual digging and building will, it is believed, soon rule out Panama and Tehuantepec, leaving only Nicaragua, yet many other routes, methods, and plans have been examined, and no portion of the isthmuses can be said to have been neglected.

Beginning at the south, we find the Atrato River, recommended by the great Humboldt, rising in the mountains of Western Colombia, and pursuing a northerly course to its mouth in the southwestern corner of the Caribbean. Although its waters empty into the Eastern Sea, its course is parallel to the Pacific coast, and only about fifty miles from that ocean. The main stem of the Andes, whose eastern slopes it drains, separates it throughout its course from the Pacific. This range is throughout this portion not of great elevation, and numerous tributaries afford, in their valleys, easy grades from the Atrato to or toward the crest of the divide.

Humboldt was told of vessels passing from the head waters of the Atrato to those of a small stream flowing southwest into the Pacific, by means of a short canal. Later examinations show that the vessels were canoes, the canal, if not mythical, was a ditch, and that a long and high portage intervened over which the canoes were dragged. Lower down the Atrato several lines of levels were run across the divide, with much care and labor, following the lines of some of the principal tributary streams entering from the westward. In these it was found necessary that the summit levels should include tunnels many miles in extent, high enough to accommodate ships with at least the lower masts left standing, and involving enormous expense. Attempts were also made to connect the Gulf of San Miguel with Caledonia Bay on the Caribbean side; and at a point farther west, to connect the Gulf of San Blas on the Caribbean with the Bayano River, emptying into the head of Panama Bay. Here again long tunnels or other formidable obstacles were soon revealed as the lines of levels were carried across.

Next came the line of Panama railroad, and here high hopes were entertained, for a railroad was already there, and the Chagres, a large stream, debouching near Aspinwall, has its source well over toward the Panama or Pacific side of the isthmus. After careful surveys on this line, it was decided that a lock canal was possible, though difficult, costing over $100,000,000, and meeting with trouble in supplying water for its summit level. A canal at the level of the sea was deemed impracticable, it being considered that the violence of the freshets in the Chagres placed it beyond successful engineering control.

The surveys of these routes had been carried on by our government, but the interest felt to-day in the Panama canal project makes it proper for me to notice other work in that locality, for French enterprise had begun to stir, and a speculative company, known as the "International Society of the Interoceanic Canal," had been formed in Paris in 1876.

I will not fatigue the Institute with the monotonous details of continuous blunders. It is the wonder of modern civilization that Count De Lesseps should have been able to raise $420,000,000 to perform a work which was known to the world, outside of France, as impracticable, if not impossible. Briefly, we will say that certain French gentlemen obtained from the United States of Colombia a concession, and having interested M. De Lesseps in the project, persuaded him to assume the leadership of the Panama sea level canal. De Lesseps, in 1879, caused the French Geographical Society to assemble, by invitation, an International Canal Congress. This congress, thoroughly under his influence as to committees, and largely so as to the body of the congress, decided, against the earnest protest of the ablest engineers in the world, to approve the sea level plan at Panama. The development of the project since that time can be read in the newspapers of the day. The company owes about $420,000,000, with an interest and fixed charges of about $22,000,000 per year. I do not understand that M. De Lesseps claims that more than one-fifth of his work is completed, and my own studies of the subject, coupled with personal observations in the earlier part of the work, convince me that one-tenth is nearer the actual amount accomplished. Lately a change has been made in the plan, and a temporary or provisional lock canal is proposed, and work is now going forward on that basis.

A glance at the comparative profile of Panama and the comments of *Engineering News* of June 2, 1888, thereupon, to be found in the appendix, will show that, although much less remains to be excavated under the lock plan than under the sea level plan, nevertheless it bears such a proportion to the effective cube already excavated that we may safely say that only thirty per cent. is done toward a lock canal.

In all these estimates we leave to one side the question of the control of the Chagres River—a problem which seems hopeless to many of the best engineers.

These facts do not surprise those who have studied the question. Great engineers warned Paris and the world of just such a disaster at the Paris Congress, while they urged Nicaragua upon their attention as being an entirely feasible, economical engineering project. We know why they were not listened to; we know how the French clustered loyally about their famous De Lesseps; how he, totally ignorant of the topographic and climatic difficulties, flushed with success and impatient of contradiction, would hearken to nothing but a French plan, executed by Frenchmen.

We cannot doubt the brilliancy of De Lesseps' vigorous intellect. His long career vouches for it. But Napoleon was brilliant, and yet committed the foolishness of invading Russia. He was great, but he had his Waterloo. De Lesseps is great, but he has his Panama.

Omitting Nicaragua, for the present, and referring briefly to the lines in Costa Rica and Honduras, the former connecting Chiriqui Lagoon with the Gulf of Dulce, and the latter crossing from the Bay of Honduras to the Bay of Fonseca, they may be summed up by stating that excellent locations for railways were found here with good harbors at the termini, but that the elevation of the mountain ranges in the vicinities made canals impossible.

Passing on still farther to the north and west, we come to the northernmost of the isthmuses, that of Tehuantepec. Cortez satisfied himself with regard to its usefulness as a land transit, and sent by that line much of the equipment arriving from Europe for his Pacific fleets fitting out for exploration and conquest. Later on, when no longer used, the world fell again into ignorance concerning it, and the ancient legends of a strait existing here gained a fresh credence until as late as the middle of the last century.

The late Captain Eads proposed, as a canal here was impossible, to take sea-going ships, loaded with heavy cargoes, out of the water, lift them upon a cradle, and carry them by rail across six hundred and fifty feet of elevation, through swamps and across streams, and finally to lower them into the water on the other side of the isthmus.

* A paper recently read before the Franklin Institute. From the *Journal* of the Institute.

The mass of engineering opinion regards the building of embankments, the management of grades and turnings, to be, under this heavy load, difficult and perhaps impossible. The mass of nautical opinion considers the lifting and carrying of heavy ships, loaded with railroad iron or other heavy weights, to be dangerous in the highest degree to the integrity and safety of the ships' hulls.

There is more than one way of avoiding breaking bulk easier and simpler than this. Ships for this isthmus trade can be fitted with interior decks on which rails are laid for cars of the lightest and snuggest construction, stowing closely together, and losing but little stowage room by their interstitial spaces. Cargo may be stowed in them, and these cars, of a size to fit a narrow gauge road across the isthmus, can be hauled out through the bow or stern ports, in a dock arranged to float the ship higher or lower as needed in order to bring its decks in succession at the level of the shore tracks.

Those cars would be run across a cheaply constructed narrow-gauge railway, and run into the hold of a ship on the other side of the isthmus, fitted in the same way to receive them.

Some little stowage space would of course be lost, but this loss would be slight compared with the enormous tolls each vessel would have to pay to allow dividends on the expensive railway needed to carry bodily a large vessel and her cargo.

This is not a specially good project; but it is one of many plans which are more feasible, economical, and sensible than the project of a ship railway.

It is a pleasure, after describing these problematical locations, these mistakes, these costly blunders, to have the privilege of describing briefly to this distinguished gathering the project of a Nicaragua canal. The value of this route has been long known. Here in Nicaragua, the backbone of the continents and isthmus, running parallel and close to the Pacific shore, sinks to its lowest point, while its eastern slope is washed by that great inland sea known as Lake Nicaragua. At this low point the divide is less than fifty feet above the level of the lake, and about one hundred and fifty feet above the mean level of the Pacific. Though the western shore of the lake is but fifteen miles from the beach of the Pacific, the lake drains through the River San Juan into the Caribbean Sea. The lake is deep and unobstructed, and the river, already navigable for light draught steamers throughout most of its length, requires but a little labor to deepen it.

Here, with such a vast water supply at the summit, with the lake itself as *a summit level*, nature seems indeed to have offered assistance in connecting the oceans. No great engineering difficulties in utilizing the lake are claimed, even by opponents of this route. There are no startling propositions connected with the plan.

Nature has decided its location, and it only remains for man to stake it out.

The lake and San Juan River *must* be the great part of the canal, no matter how the openings to the sea are made.

The route extends from Greytown on the Atlantic to Brito on the Pacific, a distance of 169·67 miles, divided as follows:

	Free Navigation.	Canal in Excavation.
Greytown to Deseado Basin...		12·37
Deseado Basin	4·00	
From Deseado Basin to San Francisco Basin		3·07
San Francisco and Machado Basins	11·00	1·73
River San Juan.....	64·00	
Lake Nicaragua	56·50	
From Lake Nicaragua to Tola Basin.....		8·22
Tola Basin....	5·28	
From Tola Basin to Brito.		3·50
	140·78	28·89

The Deseado and Tola basins are new features, brought out by the last location, as well as an increase of 2·13 miles in the length of free navigation in the San Francisco and Machado basins, or in other words, the last location has reduced the length of canal in excavation by that same distance, while the summit level has been extended from 144·8 miles to 153·8 miles.

It will require some time to complete the estimate of cost on the new location, but it may be safely stated that at least ten per cent. will be gained on the total cost based on the survey of 1885, which is $64,036,197, including twenty-five per cent. for contingencies.

The accompanying plans and profiles (Plates I., II.) explain themselves.

The principal dimensions of the canal in excavation are as follows:

Material.	Width. Bottom.	Width. Top.	Depth.
Rock.....	80	80	30
Earth....	120	180	30
Sand and loose material..	120	360	30

The minimum radius of curvature is 2,500.

The leading engineering features, in greater detail, are: The construction of two harbors at the termini of the canal, Greytown on the Caribbean Sea, and Brito on the Pacific Ocean; the damming of the waters of the San Juan River, for the purpose of raising and maintaining the level of Lake Nicaragua and the river at about 110 feet above mean tide; the formation of artificial basins at different levels by means of dams and embankments; and the use of locks, to pass from one level to another.

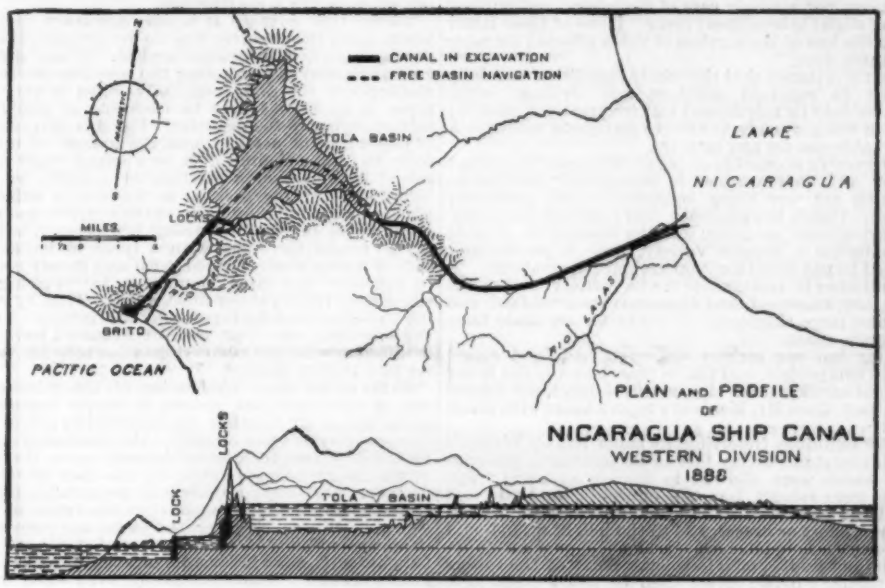
The harbors are, at present, not in good condition, but, by a comparatively small expenditure of money, can be made to offer protection to vessels of the largest class. The harbor of Greytown is now closed to vessels of more than six feet draught, but the facility for reconstruction is such that in three months from the commencement of work, at the outside, a temporary opening can be made, and material safely landed by vessels of fifteen feet draught. The further improvement of Greytown harbor is only a question of the continuation of the temporary opening. It is proposed to make this opening through the sand bar which now closes the once flourishing port of Greytown, by means of a temporary jetty of brush and pile, to furnish protection to a dredge working to cut through the bar. This jetty will also give the necessary protection for the maintenance of the cut so made. The extension and strengthening, by stone, of the jetty, and the continuation of the dredging, to form a permanent harbor, is simply a question of time.

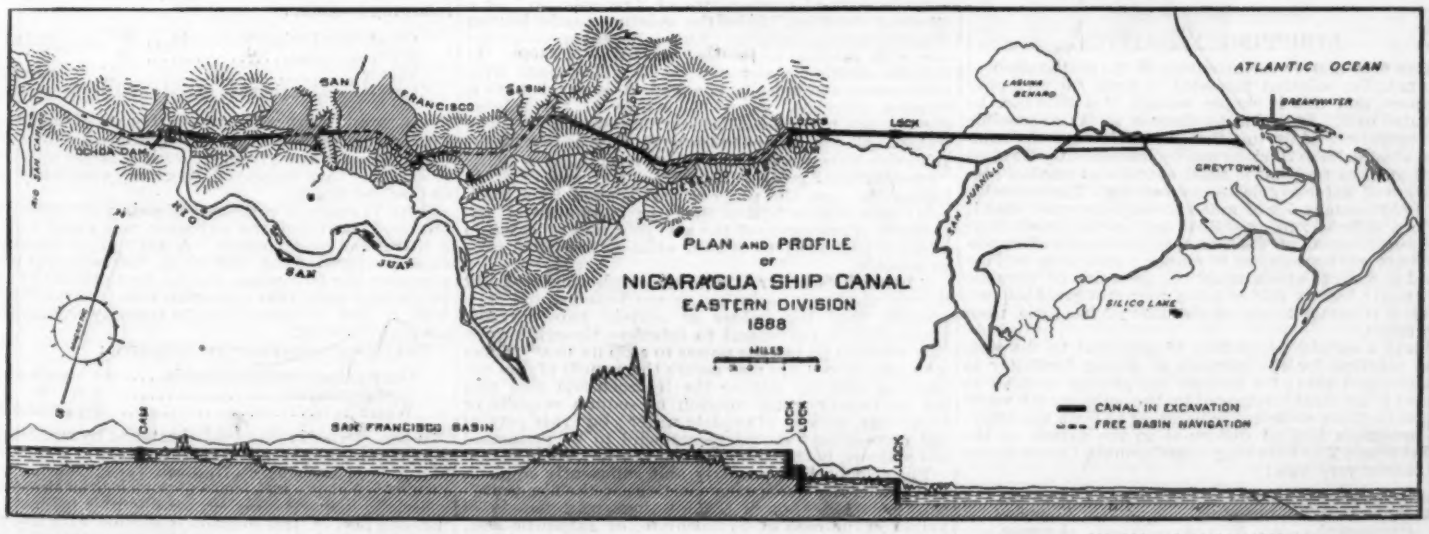
The construction of a wharf at Greytown, and of a railroad from it along the line of the canal, are the connecting links in the chain of supplies. The projected final depth of the harbor is thirty feet.

The harbor of Brito will be formed by two breakwaters, giving protection from the long swell of the Pacific, and the excavation of the harbor itself from the lowlands forming the banks at the mouth of the Rio Grande.

From Greytown, the sea level is carried to the site of the first lock, which is located at the eastern end of the valley of the Deseado. Here is a lift of thirty-one feet, into the first basin formed by damming the lower waters of the Deseado River. Through this basin we approach rapidly locks Nos. 2 and 3, which, with their respective lifts of thirty and forty-five feet, bring us to the summit level, and clear sailing through the basins of the Deseado and San Francisco, with the divide cut between, and thence into the San Juan River, across the lake and finally through the La Flor basin. Here, by means of a double lock, with a total fall of 85 feet, and again by the sixth and last lock, we descend to the sea level. This last lock has a variable lift, depending on the state of the tide, which on the Pacific side has a mean rise and fall of about six feet at present. The mean lift of the tidal lock is twenty-five feet. The locks are 650 × 70 × 30 feet.

As before stated, one of the principal features of the canal is the formation of large basins, by means of which the greater part of the canal is made a navigable body of water, instead of a narrow cut through the earth. As now projected, the first basin begins at the site of lock No. 1. This basin is formed by an embankment 1,100 feet long and 20 feet high, which maintains the level of the water at 31 feet above the sea level. A second embankment, 1,400 feet long and 86 feet high, near lock No. 2 (with a lift of 30 feet), maintains the level in a small basin at 61 feet. A third but smaller embankment, at lock No. 3, keeps the level at 106 feet. This is the summit level already referred to as extending from lock No. 3 to lock No. 4, a distance of 153 miles. The dam across the San Juan River at Ochoa, just east of the San Carlos, is 1,500 feet long by 65 feet high. Its purpose is to bank up the waters of the San Juan River, to a level of 106 feet, or 58 feet higher than at this point now. By this means a lock and a large amount of dredging is saved, and the San Juan is thus made practically navigable to Castillo, while the amount of river dredging above this point is reduced to a minimum. It will be noticed that at the dam the level is given as 106 feet. At the lake it is 110 feet, and it is proposed to give the river a fall of four feet from the lake to Ochoa, a distance of about 64 miles. Again, on the Pacific side, an embankment 2,100 feet by 80 is made across the Rio Grande. This floods the valley of the Upper Rio Grande and its tributary, the Tola. Then by cutting through the low continental divide to the lake, the summit level of 110 feet is maintained to within three miles of the Pacific Ocean. The surplus flowage is provided for in all cases, by numerous waste weirs of ample capacity. Lake Nicaragua has a watershed of 8,000 square miles.





Lake Nicaragua has a watershed of 8,000 square miles.

The Rio San Juan, *its only outlet*, discharges at its lowest stage, near the close of the dry season, 11,390 cubic feet per second, or 984,096,000 cubic feet per day.

The amount of water required for thirty-two double lockages is 129,479,968 cubic feet, or a little more than one-eighth the total supply of the lake alone, to which must be added the flow of the several tributaries of the San Juan, between the lake and the sea, and the San Francisco and its tributaries.

As this supply is *from the summit*, the danger of a dry summit level, which is so serious a question with the Panama scheme, is impossible here. It is also a favorable point that the canal will be a fresh water one.

Contrary to the general opinion, the climate of Nicaragua is an equable and almost a temperate one.

No case of yellow fever has ever been known at Greytown, and Col. Squier, in his work on Nicaragua, lays stress on the fact that, while the Mexican and Central American coasts, north and south of Nicaragua, have been visited frequently by epidemics of cholera, and are subject to yellow fever more or less every year, no epidemic of any kind is known to have visited either the Pacific or Atlantic coasts of Nicaragua.

I refer to the late expedition in confirmation of this statement.

Of the forty-nine engineers and 150 men in the employ of the company, there was not a single case of serious illness, not a single case of dysentery, and but few cases of slight intermittent fever. None of these latter caused the loss of the services of those affected for more than three days.

When it is stated that the whole expedition was daily exposed to constant wettings and dryings, often shoulder deep in marsh, and had frequently to pass the night in wet garments, the above statement would be a remarkable one for any country.

The country is capable of being developed to a great extent. It is rich in minerals, particularly gold, mines of which are now being successfully and profitably worked. Cocoa, indigo, coffee, and fruit are the principal agricultural products, but the country is capable of producing a greater variety, and is particularly adapted to the growth of market garden products.

The timber is remarkable for its variety and value. Mahogany, rosewood, and dyewoods are abundant, and even now large shipments of the latter are made from the Pacific coast.

So far but one serious objection has been raised against this project, and that is that the route lies in the region of earthquakes. Upon this subject, the following extract from Mr. Menocal's report bears with much force:

'The buildings (referring to ruins left by Walker's expedition) stand to-day just as he left them, those, at least, which were allowed to remain as ruins. The houses were rebuilt, but the churches were not, and their bare, blackened walls, standing alone and devoid of any support or braces, rise to the height of, I should say, forty feet. I have examined them very carefully, and did not find a crack in them. Some of the towers were partially blown down then by powder, and look as if they were likely to tumble down at any moment. One, especially, seems threatening to fall with the wind. Yet they stand there as they have stood since 1854. These are forty-two miles from the proposed line of canal. In the town of Rivas, two and one-half miles from the canal, they have many stone houses, and about ten years ago they completed a stone church there, which has been building for thirty or forty years—one that would be a noteworthy church anywhere—and it has never received injury from earthquake shocks, nor have the stone houses about it. I had occasion to show to the United States commissioners dams that had been built surely over 100 years ago, and are standing to-day, though not for any practical purpose; they have never been repaired, yet manifest no imperfections which can be detected by the closest examination. They were built originally for the purpose of damming up water to be used in the manufacture of indigo, and were abandoned, and have been standing there for over 100 years, perhaps."

These references show, not only that the locks and dams of the canal will not be affected by earthquakes, but that the country furnishes building materials of a superior quality.

A superior quality of lime, as evidenced by the existence to-day in an almost perfect state of preservation of indigo vats and dams over 100 years old, is obtainable at several places close to the line of the canal, and when properly manufactured, there is no question but that it will prove equal, if not superior, to many cements in the market.

(*To be continued.*)

STRIPPING NEGATIVES.

THE first matter for attention is the preparation of the gelatine solution intended to form the stripping support, and this is simple enough if a little care be devoted to it. In order to shorten as far as possible the operation of drying, it is desirable that the solution should be as thick as can be conveniently employed, that is to say, that it shall contain as small a proportion of water to gelatine as possible. The quantity of bichromate is not of much consequence provided it be not sufficient to crystallize out on the dried film; the sensitiveness of the film, *i. e.*, the length of exposure that will be required to render it insoluble, will depend to a slight extent upon the quantity of chromic salt used; but as this is not a matter of great importance, it is better to err on the side of too little than too much.

I add a certain proportion of glycerine to the gelatine solution for the purpose of giving flexibility to the stripped skin; for though the greater portion so added is no doubt removed in the subsequent washing operations, sufficient remains, or at least the effect, to produce a decided difference in the nature of the dried film. The following is the formula I have found to answer very well:

Nelson's, No. 1, "flake" gelatine.....	1 ounce.
Potassium bichromate...	10 grains.
Glycerine.....	½ ounce.
Water	5 to 8 ounces.

Dissolve the bichromate and glycerine in the water, cold, and place the gelatine to soak for an hour or two, or until thoroughly swelled. The smaller quantity of water will be the better as regards rapidity of setting and drying, but the solution will be more difficult to filter, and will also require greater dexterity in applying to large surfaces, so that it will be advisable to commence with the larger quantity. Distilled or rain water should be used if convenient; if not, ordinary tap water, rendered very feebly alkaline with ammonia, boiled, allowed to cool, and filtered through fine filter paper, may be substituted. The filtration should be performed at as high a temperature as possible in order to secure the greatest fluidity, and in the same manner adopted by the cook in making jelly, namely, by suspending a bag of wash leather, felt, or other thick, close material, in front of a clear, hot fire. Through this the solution will pass with perfect freedom, in spite of its thickness as compared with an ordinary emulsion, and it will do no harm if the operation is repeated once or twice in order to secure a thoroughly bright result. The filtration may be performed in full daylight, as the solution and jelly are practically insensitive to light, only the dried film being rendered insoluble on exposure. The mixture need not, however, be unnecessarily exposed to strong light, as the decomposition that proceeds spontaneously is likely to be hastened in that manner, and the keeping of the jelly curtailed. If kept in a cool, dark place, it will remain perfectly soluble and good for many days, or perhaps weeks, though it is in its best condition for use when two or three days old.

To use this mixture it is only necessary to pour it warm upon the negative film to be stripped, the glass having been first accurately leveled. If any difficulty be experienced in preventing the solution overrunning the edges of the plate, especially when a very thick layer is applied, it may be desirable to apply some sort of edging to the negative. For this purpose strips of paper may be pasted round the margin of the glass so as to form a shallow dish, or a slight ridge may be raised by means of a mixture of paraffin, wax, and tallow, melted together so as to form a sufficiently plastic compound; but perhaps the simplest and easiest plan, where any such treatment is necessary, is to simply go round the edges with a piece of flannel, or a tuft of cotton wool, charged with any greasy material, in precisely the same manner as in "tipping" the edges with India rubber substratum. This, by rendering the edges repellent, prevents the gelatine solution running over, and in my own experience I have found it sufficient with the thinnest gelatine solution, no matter how thickly applied.

So far as the mere application of the gelatine solution is concerned, the process is simple enough, and varies not at all, whether the negative be one of collodion or gelatine; but, actually, the treatment required by the negatives themselves depends upon the nature of the image-bearing film. In the case of collodion negatives absolutely no previous preparation is required, as the collodion itself assists the dried gelatine film to "strip." Where, however, the plates are prepared by the operator himself, and are intended for stripping, I should advise the invariable practice of polishing the glass with talc previous to coating, even when it is not adopted for other reasons, as absolute immunity from accident is thus secured. The negative having been leveled and covered with the bichromatized gelatine, is allowed to set thoroughly, and is then placed in an airy situation to become thoroughly dry. It is advisable not to resort to artificial heat, as I have found a tendency, when even a temperature far below that required to liquefy the film has been employed, to unevenness of the film after drying and insolation. It is desirable, also, that the plates be screened from strong light during drying, for the same reason, as otherwise the portions first dry become partly insoluble before the proper exposure is made, with the result that after stripping the film is apt to "cockle" on the slightest provocation.

After thorough desiccation the negative is exposed to light for a sufficient time to thoroughly harden the whole thickness of gelatine. If sunlight be available, a few minutes will suffice, but as no harm arises from extending the exposure, it is as well to give enough. In diffused light, and at this time of the year especially, two or three hours at least should be given, or even a whole day, the negative being placed in a printing frame and backed with white paper, which will reflect back the light that passes through the film. It should be turned once or twice during exposure in order that both sides may be "attacked."

After exposure, the negative is placed to soak in clean water. I prefer to use this cold, or at least at the normal temperature of, say, not more than 60° Fahr., though the gelatine is now quite insoluble even in boiling water. But I think there is a chance, with increase of temperature, of "reticulation," or a species of "frilling," from the gelatine partly leaving the glass during washing. This is more especially the case with gelatine negatives than with collodion. At first the washing water will be strongly tinged with bichromate, and must be changed at intervals until it remains colorless, when the whole of the soluble chromic salt may be considered to be eliminated. The transparent portions of the film will now be of a bright yellowish brown color, somewhat resembling a badly "pyro-stained" gelatine negative. To remove this objectionable tint, the plate is next soaked in a weak acid bath—either hydrochloric or sulphuric—of the strength of one ounce of the acid to a pint of water, in which it is allowed to remain until as free from color as possible.

A better decolorizer is aqueous sulphurous acid, but it is an unpleasant solution to use indoors, and it is possible that the holder of certain patent rights might find it convenient to interfere, though I question whether he has the power to stop its use. Those who care to venture may secure the benefit of the sulphurous acid by adding the hydrochloric acid to a ten or twenty grain solution of sodium sulphite or bisulphite instead of to plain water; but this part of the proceeding I should recommend to be performed out of doors, but certainly not in the dark room.

The acid bath, if properly and sufficiently applied, will render the transparent portions of the film practically colorless, though a faint tinge may remain of yellow in the case of hydrochloric or sulphuric acid, or smoky green if sulphurous acid be employed. When this stage is reached, nothing remains but to wash the negative in three or four changes of water, and then to drain and dry it. When thoroughly dry, the point of a sharp knife is passed round the edges of the film, one corner raised, and the whole film strips easily from the glass, forming, if the proper quantity of gelatine has been used, a stout, tough, and flexible film, perfectly flat, and in every way equal to glass for the purpose.

The only difference in the treatment required by gelatine negatives consists in the preliminary preparation by immersion in a five grain solution of chrome alum acidified with sulphuric acid. This serves the double purpose of hardening and loosening the film from the glass, though it is not absolutely necessary—at least, so far as the loosening is concerned, as the subsequent acid bath suffices to effect this. But it is a *sine qua non* that the film be rendered thoroughly insoluble. After draining, a little of the bichromatized gelatine is poured into the sink; the negative is placed on the level, coated with the stripping solution, and, in fact, treated in precisely the same manner as already described. When it comes to the operation of stripping, the only difference will be that the film clings somewhat more tenaciously to the glass than is the case with collodion, though I have never experienced any difficulty in getting the oldest and hardest films off. It is also proportionately thicker than a stripped collodion film. If any difficulty should be experienced in stripping, a bath of very dilute hydrofluoric acid, as recommended by Plener, would no doubt settle matters, though I have never tried it.

With regard to the application of this method to paper negatives, I cannot speak with the authority of practical experience beyond a few experiments which justify me in believing that with the specially prepared stripping films it is quite available.

It is, of course, a matter of necessity that the paper film should have been prepared for stripping, otherwise the adherence of the gelatine to the paper will be of such a tenacious character as to resist all treatment. But, given a properly prepared film, I do not think from my experiments that the action of the bichromatized gelatine, applied in the ordinary manner, would have time or opportunity to penetrate the negative film and reach the underlying layer of soluble gelatine. The only risk of failure lies in the chance of the soluble layer being hardened, and so cementing the image to the temporary support.

But if this method of working be not available, it is quite possible to form a stripping skin by coating a collodionized glass with the bichromatized gelatine; expose, wash, decolorize, and then, after freeing from all chromic salts, squeegee the "stripper" negative on to it, and after a short time immerse in hot water to remove the paper; then dry and strip in the ordinary manner.

There is not much outdoor work to be done at present, so those who are interested in the matter may utilize their spare time in preparing for the coming season by trying the method I have sketched, which I am quite sure, if worked with a fair amount of care, will give every satisfaction.—*W. H. Bolton, in Brit. Jour. Photo.*

HYDROQUINONE FOR DEVELOPING BROMIDE PRINTS—HYDROQUINONE SOLUTIONS FOR KEEPING.

ACCORDING to the *Photo. News*, the use of hydroquinone is becoming more extensive. It says hydroquinone with sulphite and soda has recently been strongly recommended for gelatine plates, and now we have a communication from Mr. E. Vogel, Jr., published in the *Photographische Mittheilungen*, in which he says that he now uses hydroquinone to the exclusion of all other developers for paper prints, as well as for gelatino-bromide plates. No other developer, in this writer's opinion, permits of such easy, clean, and completely certain work. This is a very strong expression of opinion from an able worker. If in the hands of others it insures a like favorable verdict, there will have to be an improvement in the method of using pyro, or that agent must in all probability encounter neglect similar to that which, for all save printing and reproduction processes, has come upon iron development.

Although now using hydroquinone only for the development of paper prints, Mr. E. Vogel encountered some difficulties and objections, which, having overcome, he describes for the guidance of those who may follow. To begin with, he mentions that with overexposure, even in slight degree, the picture, especially if the film contain much iodide of silver, readily acquires a disagreeable greenish hue. He has recently found that the tone of the picture depends essentially upon the amount of carbonate of soda present in the developer. With a solution prepared according to the following formula:

Crystallized sulphite of soda...	40	grains
Hydroquinone.....	5	"
Water.....	2¼	ounces
Crystallized carbonate of soda.	50 to 150	grains

it was found that when the carbonate of soda was in small proportion—*i. e.*, up to 75 grains—the tone of the print was always greenish; with a larger proportion of soda the tone improved, becoming eventually black or blackish brown.

With 75 grains of carbonate of soda good results were obtained, provided the exposure was exact, but not if there was overexposure. A very great increase of soda produced some blistering. Carbonate of potash possesses the advantage, for the development of paper prints over soda, that a greenish tone is never obtained with it, and is therefore to be strongly recommended for positive work.

The following mixture was prepared:

Crystallized sulphite of soda.....	40	grains
Hydroquinone.....	5	"
Water.....	2½	drachms

When perfectly dissolved (hastened by standing the bottle containing the ingredients in warm water), 50 to 54 grains of carbonate of potash was added. A larger proportion of the carbonate of potash than this causes, especially in warm weather, considerable frilling. For use, one part of this solution is diluted with five parts of water. The developer, in the concentrated form as above, keeps good for a long time; when diluted to the strength given for use, it remains good only for about two or three weeks. The strong solution is, on account of its keeping properties, very convenient for use on a journey. When using with gelatine plates, Mr. Vogel states that he obtained very good results with this formula, and that it may be especially recommended for instantaneous negatives on account of the very full amount of detail brought out by it. The development also proceeds quickly.

It is important to notice that the sulphite of soda is in good condition: has not effloresced by keeping. On account of the greater keeping properties of a concen-

trated solution he now gives the formula in the following shape:

Crystallized sulphite of soda.....	160	grains
Hydroquinone...	20	"
Water	1¾	ounces
Crystallized carbonate of soda....	300	grains

The precaution previously mentioned of insuring that all the hydroquinone is dissolved before the addition of the alkali must of course be observed. For use, two parts are diluted with five parts of water.

Both the potash and the soda developer can be varied so as to suit differing characteristics. For plates working with too much vigor, less water may be used in the developer; while on the other hand, plates yielding an image deficient in vigor may be treated with a solution to which more water has been added.

An item having an important bearing on the general extension of the use of hydroquinone is the much lower price at which it is now procurable. Mr. Vogel tells us that in Germany it is now less in price than pyrogallic acid, the cost of a kilo being thirty marks, equal to about tenpence per ounce of our money.

ALUMINUM ALLOYS.

AT a recent meeting of the Cleveland Institution of Engineers, held at Middlesborough, Mr. William Boby (London) read a paper which deserves the special attention of ironfounders, wrought iron makers, and steel manufacturers, as showing the value of the addition of small quantities of aluminum to iron or steel in course of manufacture. Aluminum is apparently destined to play an important part in metallurgy, as applied in practical matters, and the uses of the Cowles process may be extended over a wide field. In this country the process is being carried on by the Cowles Syndicate Company (Limited), at Milton, near Stoke-on-Trent, and the inventors are Messrs. Eugene and Alfred Cowles, Lockport, U. S. A.

Mr. Boby, in describing the Cowles furnace, said it consisted of an oblong receptacle, the internal dimensions being 5 feet long, 3 feet 3 inches deep, and 20 inches wide, and it was lined with firebricks; into the ends were built cast iron tubes, inclined for convenience in working the electrodes. The top of the brickwork was covered by iron plates, and an iron cover was placed over the mouth of the furnace; in the cover was a hole through which the gases issued as they were driven off by the heat in the furnace. As a matter of fact, they had a covered furnace provided at its ends with two electrodes, which were capable of being easily moved apart. The two electrode rods were connected to the two leads of the dynamo, the cable connected with the positive pole sending the current into the left hand electrode, whence it traveled through the charge, where it met with the resistance which caused the heat, and passed by way of the other electrode into the cable connected with the negative pole of the dynamo. The resistance was caused by particles of carbon, which were mixed in the charge; thus the current acted independently of the structure of the furnace itself, acting simply on the charge, which was embedded in charcoal. Mr. Boby then described at length the mode of manipulating the furnace, and remarked that the temperature produced in it was probably the highest ever obtained in England, and no method had yet been devised for obtaining any idea of it.

He thought the metals in the charge were volatilized after the more volatile oxygen had been driven off, and rising in that condition from the neighborhood of the electrodes, met the cooler portions of the lining and became precipitated, the metallic alloy being found in the form of a slab on the charcoal lining below the electrodes. It was assumed that the aluminum combined with the metallic base of its alloy while both were in the gaseous condition. The production of aluminum at the Milton works was at present about 200 lb. of actual aluminum per day of twenty-four hours when the aluminum was produced as an alloy with copper, or, if the alloy had iron for its base, a less amount was obtained—about 150 lb. The average number of runs per day of twenty-four hours was twelve to fourteen, so that they obtained 15 to 18 lb. of pure aluminum (embodied in the alloy) per run when producing bronze.

The dynamo, the speaker said, furnished probably the largest amount of current of any machine in existence, its normal output being 5,000 amperes at a tension of 60 volts. It was built by Crompton & Co., and was of the constant current type, with drum armature. As regarded the use of aluminum, he might say that the ferro-aluminum was entirely for mixing with other iron, either cast or wrought, or (which appeared to be its greatest field for usefulness) with cast steel. When the amount of concentration exceeded a certain amount the alloy became non-magnetic, and this was a property which might possibly be found to be of practical value.

Another good property of aluminum as produced by the process was the quality of equal diffusion—a ladleful of molten steel would be impregnated with a minute proportion of aluminum if a small quantity of the broken-up alloy were placed in the bottom of it before the steel was poured in. The use of ferro-aluminum, said Mr. Boby, had not up to the present obtained any large dimensions in this country, but in the United States it was already largely employed. The effect of the alloy upon steel was to raise its elastic limit and ultimate strength, while it lowered its melting point and facilitated the making of sound castings, the last named point being one of great importance. This alloy was employed by the makers of mitis castings in America, who obtained it from the Cowles Company, at Lockport. Mr. Boby's partner, when spending a few days there some months back, saw an order come in for ten tons, and, when it was borne in mind that one-tenth of one per cent. of aluminum was an ordinary proportion to add to steel castings, it would be seen that this represented operations of some magnitude.

The effect of ferro-aluminum upon puddled bar iron had been investigated by a member of the firm of P. & W. Maclellan, Glasgow, with most satisfactory results, the tensile strength being raised to 31 tons with 23 per cent. elongation, while the quality was equal to that of the finest mild steel. Mr. W. J. Keep, Michigan Stove Company, U. S. A., one of the largest stove making concerns in the States, bore testimony to remarkably beneficial effects of ferro-aluminum as applied to cast iron. English firms were commencing to use this alloy, and no doubt its employment would rapidly extend. Some of the best known steel founders were using it in England. Dealing with the bronzes, the author said that the highest proportion in which aluminum was useful in copper was a trifle over 10 per cent., and a little silicon was found to be advantageous. In this highest grade of aluminum bronze the elastic limit and breaking strain were coincident, and reached a very high figure, as high as 50 tons per square inch; and with very careful manipulation and well selected copper a small test bar had withstood a strain of close upon 60 tons. If more aluminum than the above named were added, the alloy lost strength by becoming brittle, and the 10 per cent. grade was probably the strongest alloy known; its specific gravity was low and it worked splendidly. It could be turned with the same form of tools which were ordinarily employed for working mild steel. It could be rough turned or planed with the ordinary diamond-nosed tool, with plenty of lift to its edge, and finished with a broad-nosed tool and a very quick feed, which put a polish upon it like a looking glass.

It could be forged hot to any desired extent, as it was extremely soft when at a bright red heat, and the forging did not affect its properties except to a very slight extent, it being, perhaps, slightly more ductile afterward. There was another alloy which promised to be of a more general application even than the bronze, viz., aluminum brass, which possessed, perhaps, the most surprising qualities of any of the aluminum alloys. They had a mixture of which one-third of its entire weight was spelter, while the other two-thirds were 5 per cent. aluminum bronze—in fact, the same ingredients as went to make up ordinary brass, except that they had about 3½ per cent. of aluminum, and this last small addition produced the astonishing result of a strength which sometimes reached 95,000 lb. tensile, 85,000 lb. being an ordinary strength; its elastic limit was then very high, its ductility ranging from 8 to 4 per cent., while its average transverse strength reached the extraordinary figure of 73 cwt., tested on a bar 1 inch square and 12 inches long between supports.

Both the bronze and the brass were practically unaffected by salt water. They promised to be of extensive application for bearings, for light, strong, and incorrodible parts of machinery, for distillery and brewing apparatus, pumping purposes, for propellers, for portions of ships now forged at great expense, for artillery. Mr. Boby remarked that a great deal of nonsense had been printed in the public papers about pure aluminum, that it was as strong as steel and one third its weight. In reality, it was not more than one-third as strong as steel, and was notoriously difficult to cast, and its use on any very extended scale was doubtful. The aluminum brass, however, did not offer these difficulties, being easily cast, and there seemed to be no reason why it should not take the place of more expensive bronzes.

WATER SUPPLY TO STEAM BOILERS.

By THOMAS T. P. BRUCE WARREN.

IT is a common practice to use waste steam for heating water before injecting into a boiler; in most cases the exhaust steam from the cylinder is condensed, and flows back again into the boiler. If the cylinder be lubricated with a compound oil containing a saponifiable oil or fat, serious consequences are likely to result, in consequence of "pitting" of the plates. Although this is so well known, I am not aware of any published statement on the chemistry of this subject.

A few years ago a sample of water drawn from a boiler was said to contain grease. Both the water and deposit from the boiler were repeatedly examined, with negative results. A sample of *scum* collected from the surface of the water was subsequently examined in the following way:

A weighed quantity of the dried scum was digested in warm dilute HCl; the solution, on cooling, was coated with a dirty, unctuous film. On filtering and washing, the soluble matter was removed, the dried residue was treated with CS_2, which, on evaporation, yielded four per cent. of a white solid fat, consisting principally of stearic acid.

As these earthy soaps float, they attach themselves to the boiler plates, and, being decomposed, leave a greasy spot on the plates. This grease prevents the water from coming in contact with the plate, which probably gets more strongly heated; however, the plates rapidly corrode at these spots, but how far the action is chemical or mechanical, or combined, it is not so easy to decide.

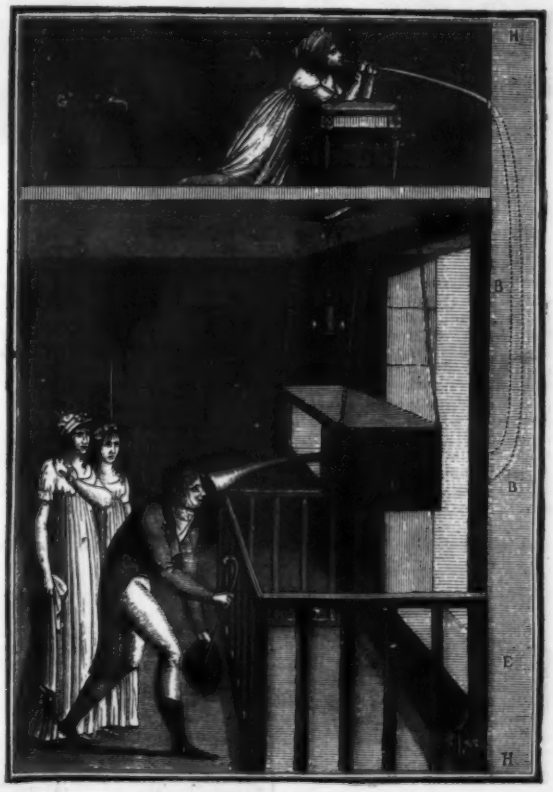
Petroleum or other non-saponifiable lubricators should be used in every case where it is intended to utilize waste steam, direct, in heating the feed water.

I brought this corrosion of boiler plates under the notice of the late Mr. Keats, chemist to the Metropolitan Board of Works, who said that this corrosion was due to sulphate of lime. If sulphate of lime be present in this scum, the corrosion may be more easily explained, as the generation of sulphurous acid is by no means improbable.

Chloride of magnesium, if present in large quantity in a feed water, will generate HCl, and which, escaping with the steam, will rapidly corrode the iron pipes conveying the steam. A water residuum heated to 260° F. evolves large quantities of HCl, if much chloride of magnesium be present. Probably the reason why this corrosion is not perceptible with marine boilers is because the steam pipes are made of copper.—*Chemical News.*

THE INVISIBLE WOMAN.

AT the end of the last century and the beginning of the present, a very curious experiment, and one which was looked upon as marvelous by the credulous, was wonderfully popular at Paris. The representation took place at the old Capucines convent. The spectator entered a well lighted hall in which, in part of a window, there was a box suspended by four brass chains attached by bows of ribbon. The box, which was surrounded by a grating, was provided with two panes of glass that permitted of seeing that it was absolutely empty. To one of the extremities was fixed a speaking trumpet. When a visitor spoke in the latter, he was answered by a hollow voice; and when he placed his face near the box, he even felt upon it the action of a mysterious breath. When he presented any object whatever in front of the mouthpiece, and asked the voice to name it, an answer immediately came from the speaking tube. The box was suspended freely from the ceiling, and it could be made to swing at the extremity of the chains; it was empty and isolated in space. People were lost in conjecture as to the secret of the experiment. Among the unlikely theories that were put forth was that of the invisibility of a person obtained by unknown processes.



THE INVISIBLE WOMAN.

As usual in these kinds of impostures, there was here merely an ingenious application of a scientific principle. A physicist, E. J. Ingennato, revealed the mystery in a pamphlet published in 1800 under the title of the Invisible Woman and Her Secret Unveiled. This tract, now rare, had for a frontispiece the engraving which we reproduce herewith and which explains the whole experiment. The invisible woman of the Capucines convent was named Frances, and the following is the explanatory legend appended to the original engraving:

"Questioner: 'Frances, what is this that I have in my hand?'

"Frances (after looking through the little peep-hole, D): 'A stick with a crooked handle.'

"The entire assembly at once: 'It is incomprehensible!'"

Ingennato, in his pamphlet, explains that above the ceiling there was a low, darkish chamber, in which Frances was concealed, and that she looked at the object presented to her through a small aperture, D, which was skillfully hidden by a hanging lamp, and then answered through the speaking tube, B B B, hidden in the wall. The sound traversed a space of about six inches, that separated the speaking tube from the speaking trumpet.

On account of its ingenuousness, we reproduce the explanation of the plate as given by Ingennato:

"A. The alleged invisible woman. B. Tube through which she causes the words to be heard, whose sound reaches the second aperture of the speaking trumpet. C. Second aperture or second bell of the speaking trumpet. D. Small bracket fixed to the ceiling in order to support a useless lamp, and containing an aperture. This latter, which is rendered more visible here than it really is, corresponds to another one in the ceiling, and it is through the little peep-hole that it forms that the invisible woman looks at the objects before naming them. E. Grating that surrounds the box, reported to conceal the invisible woman. Although this is low in the plate, it extends up to the ceiling. F. Box supposed to contain the invisible woman, and forming the principal part of the mystery. G. Upper chamber, in which the woman called invisible is really located, although the contrary is asserted to the public. H. Wall of the upper and lower chambers, in which an opening has been made for the insertion therein of the tube, B. This latter is here shown exposed, although it is hidden by a thin coat of plaster containing imperceptible apertures opposite the tube's lower extremity, which is expanded."

The invisible woman uttered sounds with caution, without well articulating them. She exercised hourly in giving her voice deadened inflections, which were re-enforced by the metallic tube of the box. It seemed as if the sounds really came from the wonderful box.

On consulting the journals of the time, we find numerous hypotheses regarding this experiment, which truly struck the credulity of the public.—*La Nature.*

CIVIL SERVICE IN TONKIN—EXAMINATION OF THE LITERATI.

THE examination of the literati, which takes place every three years at Nam-Dink, is designed for the recruiting of officers of state, which, in Annam, form a true class by themselves—an aristocracy which enjoys very enviable prerogatives and privileges. It is easy to imagine what animation is given to the city by this avalanche of would-be bachelors, licentiates, or doctors, of all classes and ages, sweeping down from every part of Annam. As soon as the session opens, quiet again reigns. The candidate installs himself in a booth in the space reserved for him, and does not leave it during the entire period of the examination, say for nearly a month. As may be seen, these conditions distantly recall those imposed upon our lodgers of the School of Fine Arts. After the session has closed, the examiners, who have come from Hue with great pomp, classify the compositions, and, on the day of the ceremony, the names of the new lettered persons are proclaimed by a crier standing upon a stage.

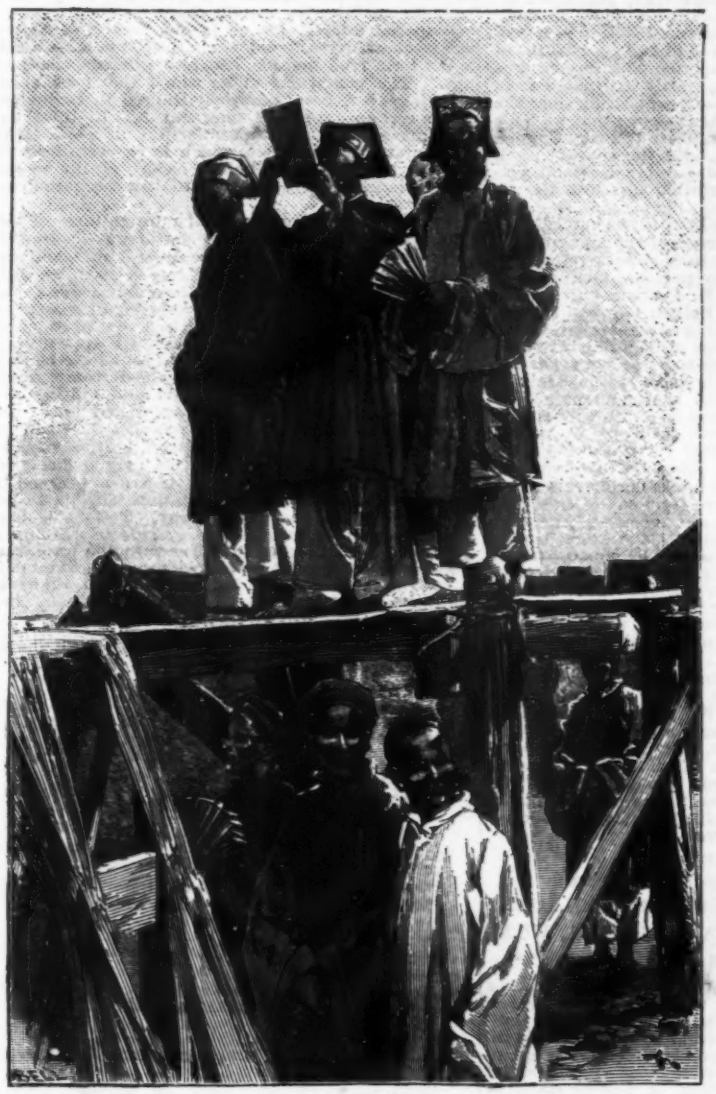
It is this ceremony, in its various aspects, that is represented in our engravings, the legends accompanying which permit us to dispense with commenting upon them one by one.

THE PRESIDENT AND JURY OF EXAMINING MANDARINS IN THEIR OFFICIAL COSTUMES.

The camp of the literati is located at about a mile from the city. It is entirely constructed of lodges and bamboos, and this gives it a very peculiar appearance. At the entrance, to the right of the grand avenue that leads to the royal pagoda, there stands a stage protected by mats and ornamented with strips of embroidery; it is the official tribune. Upon the left side of the same entrance, the examiners, in full dress, surrounded by their servants and parasol bearers, are seated upon their stools. To the left of the stage of honor, upon a high flooring forming a tribune, and overlooking the noisy crowd that presses around, the criers, provided with huge speaking trumpets, proclaim the names of the elected.

THE MANDARIN EXAMINERS DURING THE SUMMONING OF THE ELECTED.

CEREMONY OF THE PROCLAMATION OF THE LAUREATES IN THE COMPETITION OF LETTERS AT TONKIN.



SUMMONING THE LAUREATES.

THE PRESIDENT OF THE JURY.

CEREMONY OF THE PROCLAMATION OF THE LAUREATES IN THE COMPETITION OF LETTERS AT TONKIN.

As their names are called, the laureates step out from the crowd, make a salute, and take a place to the left of the stage of honor. When the group is complete, there is given to each licentiate the costume of the literati, which consists of a black tunic, a square cap, and a pair of Chinese sandals.

Thus equipped, they proceed to the royal pagoda and arrange themselves in the court. Then, and after they have received instructions from the mandarins who accompany them as to the manner of paying homage to the king, the ceremony of investiture begins. The royal envoy places himself first, then come the other mandarins, and finally the group of elected.

The crowd which has invaded the pagoda retires to the sides, and, during the profoundest silence, a mandarin pronounces an invocation. The mandarins alone prostrate themselves to the earth five times in succession, and then begin again with the group of the elected. After this, all return and face the tribune of honor.

As may be seen, this ceremony is wanting neither in originality nor grandeur. This year especially, the presence of high functionaries of the French protectorate, and, at their head, Mr. Parreau, the resident general, who, through an interpreter, delivered an eloquent address to the literati, gave the fete of the 7th of September an unusual luster. This official intervention, too, produced an excellent effect upon the lettered caste, which, up to the present, has always put itself in opposition to the protectorate, which it has accused of being the enemy of Annamite manners and customs.

This proves that it is easy for whoever wishes to do so to cause a cessation of these regrettable misunderstandings. Moreover, when the idea shall have occurred, as it did under the present circumstances to Mr. Parreau, to join with kind words a few little presents such as watches, music boxes, etc., we may hope to see before long silent or declared hostility give place to a frank and durable friendship.—*L'Illustration.*

EXAMINATIONS OF THE LITERATI AT HUE.

THE *Annales de l'Extreme Orient* says that the examinations of the literati consist exclusively of written tests, and these, five in number, all relate to literature and philosophy. They take place at ten days' intervals, and each lasts a day—from six in the morning to six in the evening. The entire session, therefore, lasts a month, and, during this time, the examiners themselves, cloistered like pupils, do not leave the inclosure in which the compositions are written. Properly speaking, the candidates are imprisoned, that is to say, each one remains in a small, narrow, and solitary booth without any other aid than his memory or the inspirations of Buddha. Royal soldiers, armed with lances, play the role of masters of study, and see to it that no communications take place. The orders are very severe, and very severe, too, would be the punishment of him who forgot them. The day that we went to see the candidates, quite distant from Hue, on the Quang-Try route, it had rained much for the past forty-eight hours, and more than one booth had been invaded by water; but the unfortunates did not seem to be much concerned about it. Squatting upon the ground, and bent over, with bamboo pencil in hand, silent and attentive, they were working away. There were thirteen hundred of them; but, out of this large number, only eighty-six were received, twenty-nine as *cu'-nhau*, that is, licentiates, and fifty-seven as *tu-tai*, that is, bachelors.

The classification of the compositions establishes the hierarchical order. The notes are "good," "pretty good," "passable," and "bad." "Pretty good" suffices to make a licentiate. The judges are possessed of rare impartiality. The son of the president of the council of ministers was refused, and the same was the case with the son of the Kin-luoc (viceroy of Tonkin).

In order to be a bachelor, it suffices not to have a "bad" note, or "black ball" as we would say. The number of candidates to be received is fixed annually by royal edict. Last year (1888), the number of licentiates for the province of Hue was thirty-two, and that of the bachelors had to remain in the proportion of one to three with that of the first. Hue is the center of examinations for the provinces of Thua Thien (Hue), Quang-Nam, Quang-Bin, and Quang-Try.

Each of the five compositions is corrected as soon as finished, and every candidate declared inadmissible is not allowed to undergo the succeeding tests. On the morrow, therefore, considerable of a selection occurs, and the ranks soon perceptibly thin out.

The candidates are submitted to certain rules. One of the most curious of these is the one that expressly forbids the writing, in their entirety, of the characters of the names of ancestors or of royal temples. He who should render himself guilty of this crime of irreverence not only would be at once excluded from the examinations, but would also be condemned to fifty strokes of the rattan, which, however, as explained to us by a magistrate, are redeemable at the rate of a half-ligature per dozen. This is as much as to say that, during the course of the day, it is possible, for about twenty-five cents, to satisfy one's self with the luxury of this little literary shamelessness.

The forbidden characters are posted up on the eve of the first composition, so that no one shall ignore them.

In addition, if the compositions of two candidates should contain an identical phrase, the writers would be, *de jure*, excluded from the competition, as they would be held to have copied one from the other.

After the third test, the received candidates are obliged, for greater security, to pass a counter examination serving to verify whether each has really written his compositions alone and unaided. It is not till then that the tests are declared closed, and the *cu'-nhau* (licentiates) are admitted, without a new examination, into the mandariney of the seventh degree.

The examining jury is composed of high mandarins selected by the king; but the president and vice-president must always be *tan-si* (doctors). Their notes alone count in the final classification. Those of the assessors, who may be simple licentiates, serve only for the preliminary eliminations. In most cases, the compositions consist of an analysis of a phrase taken from the classic authors, or of the development of some philosophic thought.

The proclamation of the results of the competition takes place with great solemnity. Gala seats, so high that they have to be reached by genuine ladders, are prepared for persons of note, either Europeans or court mandarins. At the side, from the top of a stage, a crier announces to the crowd the names of the elected. The person named then takes his place in a separate group. When the group is complete, all the candidates are admitted into a special hall, and it is here that are distributed among them the little felt-soled sandals and the caps that become their university insignia and the distinctive emblems of their caste.

THE OPENING OF THE PASTEUR INSTITUTE.

"WE cannot refrain from expressing some regret that the encouragement of scientific research should be one of the things which they do better in France than among ourselves." With these words, trenchant enough if heeded by those in authority on whose ears they may fall, the *Times* concludes a leader on the inauguration of the Pasteur Institute by the President of the French Republic. Such a ceremony naturally suggests two distinct points for consideration: (1) the object of the institution thus inaugurated; (2) the interest attaching to the ceremony.

The Pasteur Institute is remarkable among all others in being the best form of monument ever erected, and at the same time in its being raised during the lifetime of the distinguished man of science in whose honor and for the furtherance of whose work it was designed. That the debt which the community owes to M. Pasteur will never be paid, nor even adequately acknowledged, needs no insistence; but we may be excused if we dwell upon this point a little, for in the multifarious and different battalions of the workers in the army of science there may well be some whose particular work has not quite brought home to them their obligation to him.

The most remarkable characteristic of M. Pasteur's work, the one which places it on so unique a pedestal, is the fertility of its results in every direction. To have elucidated at once the causation of most forms of fermentation, and the causation of most forms of acute febrile disease (this last leading to the infinitely precious invention by Sir Joseph Lister of antiseptic surgery), is on the chemico-biological side of natural

science a feat of as great abstract value and of greater immediately practical worth to the community than any one, or even two, of the greatest epoch-making discoveries of physical science. If it were not for the lamentable consequences of the apathy with which the British public regard science and its contributions to their health and wealth, it would be sadly amusing to read, as any one may do in even well-founded prints, the lay opinion that M. Pasteur is but a hydrophobia curer, and possibly a slightly more successful one than McGovern, the Irish quack. The flame of popular knowledge of current science always burns most unsteadily, and any sensational wind makes it flare for a short time, and then it sinks almost extinguished. It has thus been with the most recent work of M. Pasteur; and so we find at the inauguration of the Institute the wide subject of the chemico-biology of disease processes was subordinated to the representation of the existing condition of our knowledge of the treatment of rabies.

Although, considering the national importance of the general principles of M. Pasteur's work, this preponderance of attention given to one subject may be regretted, it nevertheless must be admitted that a specific instance is more easily "understanded of the people," and may consequently more energetically drive home the wedges of scientific truth. To M. Grancher was most justly accorded the very agreeable task of expounding in a few simple and unadorned sentences the results of the anti-rabietic treatment of M. Pasteur. Though rabies, or hydrophobia, has always occupied such a special position in the public mind, this has not prevented the application of the general principle of public ingratitude; and we are therefore in nowise surprised to find that the benefactor who arose, and, at his own risk and cost, attempted to remove such an evil, should have been received with calumny and misrepresentation. The consolation afforded by the unerring verdict of time rarely comes—as in the present case it fortunately has to M. Pasteur—before the benefit-conferring Prometheus is past receiving it.

M. Pasteur has always borne the monstrous attacks made upon him with such dignity and composure, that the summary by M. Grancher of the great works suggested by him must have been an intense gratification and recompense.

Our sympathy with his pleasure is unfortunately alloyed with regret that of recent years health has been denied him for the perfect enjoyment of his renown.

The announcement by M. Pasteur in 1885 (the year of the epidemic of rabies in London) that he had not only succeeded in rendering dogs refractory to rabies by means of prophylactic inoculations, but had also with the same material attempted, and apparently successfully, the curative treatment of two human beings, marked the commencement of a widespread application of his now fairly well-known methods.

From the first, M. Pasteur recognized the effect that such an announcement would have upon the public mind, and, in addition to forming a resolution only to treat assured cases of rabies (a resolution he had ultimately to abandon on the grounds of humanity), arranged the facts of his work in such a manner as to provide for complete statistical accuracy in his records.

By his prescience we are thus placed in possession of an overwhelming series of facts relating to persons bitten by rabid animals. He arranged those who came to him under these circumstances into three categories.

In the first (Class A) he placed persons bitten by animals indubitably proved to be rabid by the results of inoculation from the spinal cord into normal animals.

Secondly (Class B), he grouped together those cases in which the state of the animal, though not tested by experiment, was nevertheless certified to have been rabies by a veterinary surgeon.

Finally, he constructed a third order (Class C), in which were collected those cases in which, owing to escape, etc., of the dog or animal attacking, no precise information as to its condition could be obtained, but only a presumptive suspicion that it was rabid.

Before we review the figures derived from these three classes of patients, it is important to gauge the character of the statistics of the general mortality from the disease with which they have to be compared. It is only since special attention has been drawn to rabies through M. Pasteur's work that trustworthy statistics have been forthcoming. In former years estimates of various kinds were from time to time prepared, but while some authors took only cases of the most virulent kind, and consequently obtained exceedingly high death rates among those bitten, others accumulated large numbers of instances the details of which were most imperfectly ascertained, and the mortality percentages thus deduced consequently utterly untrustworthy. The severest test that could be conceived for genuine criticism of M. Pasteur's method is obviously the comparison of the death rate in his Class A with that among persons, not his patients, proved to have been bitten by rabid dogs by the fact of at least one of those attacked by the animal dying of the disease. Such a comparison is now fortunately possible. The probability of rabies following the bite of a rabid dog is now definitely ascertained to be from 15 to 16 per cent. of those attacked.

Now the death rate in M. Pasteur's Class C is no more than 1·36 per cent., even including every fatal case—that is, inclusive of those persons who develop the disease during the first fifteen days after the bite. The rigid comparison of these two death rates may well afford M. Pasteur the satisfaction of feeling that he has saved a number (to be counted by hundreds rather than tens) of his fellow-creatures from the most agonizing of deaths, and an enormous number from the worst of apprehensions.

For general biological science the next most interesting statistics are those which seem to reveal the mode of action of the curative and prophylatic inoculations. M. Pasteur's explanations of the beneficial effects of the material inoculated was that the nerve tissue contained not only the microbes, the causative factors of the disease, but also their metabolic products, and that these latter by accumulation inhibit the growth and spread of the organisms. If, therefore, these products were injected into the blood stream in sufficient quantity, he believed that the animal so treated would be protected from the malady. In this country Dr. Wooldridge had already proved experimentally the occurrence of such a process in the case of anthrax or splenic fever. Now the accumulated experience of M. Pasteur's laboratory goes very far to establish this theory for rabies also. Thus in Russia, where rabies is frightfully prevalent by reason of its being endemic among wild (wolves notably) as well as among domestic animals, the figures obtained from the respective inoculation stations are most striking:

	Odessa death rate per cent.	Moscow death rate per cent.	Warsaw death rate per cent.
1888. "Traitement simple" (*i. e.*, small quantities injected)	3·39	8·40	8
1888. "Traitement intensif" (*i. e.*, large quantities injected)	0·64	1·60	0*

It is abundantly evident from these figures that successful protection is due to the energy and frequency with which inoculations are practiced, or, in other words, to the quantity of protective material injected. While we cannot too heartily congratulate M. Pasteur on his triumph in finding a cure for this miserable disease, we feel very glad that, since his work has established the true nature of rabies and its mode of propagation among animals and men, the French authorities have at last awakened to the fact that there is no disease which can be more successfully prevented by legislation. M. Grancher exhibited a chart showing the immediate effect of preventive legislation in reducing the prevalence of the malady in the department of the Seine. For us, our own experience of the measures whereby the disease was temporarily extirpated from London (though now, of course, reappearing since the relaxation of the restrictions) is so strong that we hope this additional evidence will induce our Privy Council to apply such measures throughout the country; and having thus stamped out the disease in England, prevent by suitable contra-importation measures the reintroduction of the disease.

So much for the work of the Institute as immediately in operation. The special interest of the inauguration ceremony is noteworthy. We have already referred to it as being in part due to the personal monument it establishes to the genius of M. Pasteur, but it has a more particular interest for British national science. It lies in the fact that here we see an institution erected for the national purpose of scientific investigation into the causes of diseases and their mode of prevention. We see, moreover, the head of the executive government, in company with the members of his cabinet, personally giving to the movement his cordial interest and support. It must make us all wonder when our government will cease to regard the social and political importance of scientific investigations with other than an absolutely ineffective interest.

At present, for scientific investigations of this kind this country and its government are positively dependent upon the charity of a private laboratory, that of the Brown Institution, the income of which, utterly inadequate, is very imperfectly helped by the defrayal on the part of the government of simply the immediate expenses of the work done for them. And at the same time we wonder when our government will remove the disgraceful legislative hindrances to British scientific work. Finally, we may ask, When shall we see the scientific millennium of an English ministry taking an immediately personal interest in the welfare and support of such an institution? We can only conclude in the spirit of the words of the *Times* with which this article begins; and hope that, if it is generally appreciated how the lead has been taken from this country by France, at least an effort will be made by those who are responsible for the discredit thus forced on us to remove the blot by organizing a somewhat similar institution in England.—*Nature.*

* "Intensif" treatment for last sixteen months—no death.

[Continued from SUPPLEMENT, No. 682, page 10900.]

YEAST: ITS MORPHOLOGY AND CULTURE.†

By A. GORDON SALAMON, A.R.S.M., F.I.C., F.C.S.

LECTURE III.

IT has been shown by Pasteur that the conditions under which yeast is capable of exciting alcoholic fermentation are referable to defined limits of temperature, and to the presence or absence of a certain amount of free oxygen. Beyond a determinate quantity of the latter, which is necessary to the fulfillment of the vital functions of the plant, it may be stated in general terms that its increase during the growth of yeast is accompanied by a decrease in the alcohol-inciting power of the latter. This, however, is not the only effect which the presence of free oxygen is capable of exerting upon yeast. It can produce a remarkable alteration in its morphological appearance. The observation did not escape the attention of Pasteur, although it is but remotely connected with those problems in fermentation with which he more directly concerned himself. He did not, therefore, do more than notice the point, and it was reserved for Hansen to discover its special significance with respect to the saccharomyces group.

If a preparation containing sugar, such as sirup, jam, or preserves, be exposed to the action of a non-sterilized atmosphere, it will, as is well known, become, in course of time, covered with a coherent film, which will gradually become thicker and tougher until it ultimately assumes the texture of a felt, upon the exterior surface of which patches of green color soon make their appearance. This felt, as before stated, is composed of interlaced hyphæ, constituting a mycelium from the surface of which spring sporophores bearing the reproductive organs of the particular fungus which has thus developed. The most usual growth in such a case is that of the common mould, *Penicilium glaucum*. It must not, however, be supposed that it is only the moulds, or indeed the mycelial-forming fungi, which can thus develop upon the surface of a suitable liquid. The development is generally that of *penicilium*, because the latter is almost always present in the atmosphere; but if its place were taken by other fungi, they would be capable of manifesting analogous, if not exactly similar, phenomena. Indeed, it has been generally proved that fungi which are capable of forming hyphal—that is true—mycelia can develop in this way. They do not form a thick felt-like *penicilium*, but they may cover the surface of the liquid with a thin cohesive veil. The term by which we shall recognize such a formation is that of a "mothering,"* the appearance of which will vary with the organism which gives rise to it; but it may be formed by pure cultures of various fungi, and by cells resembling saccharomyces, and, indeed, by true species of saccharomyces themselves. Of those cells resembling the latter, it has been pointed out by Hansen that it is an error to suppose that the "mothering" of beer, developed when the latter is exposed for a long time to the action of the air, is composed exclusively, as hitherto believed, of *S. mycoderma*, and he has proved that it may contain several distinct species. Moreover, such cells are not necessarily devoid of fermentative power. *S. mycoderma* forms a "mothering" exclusively by means of those cells which are sown upon the surface of the liquid; but *Monilia candida*, which has the power of fermenting cane sugar without previous inversion, can fall to the bottom of the liquid, where the cells incite alcoholic fermentation like bottom yeast; they can then rise to the surface by the aid of the disengaged carbonic acid gas, and can subsequently develop a typical cotton-white "mothering," differing in appearance from those produced by other moulds, excepting *Oidium lactis* and one or two other particular species. *Torula, S. apiculatus*, and *S. exiguus* can also produce "motherings" having specific characteristics.

Of special interest for us is the discovery that the true saccharomyces, as defined in the last lecture, which do not form true mycelia, can also produce "motherings." The conditions under which they form them have, however, been shown by Hansen to be limited within certain ranges of temperature. In these circumstances they exhibit peculiarities which go far to corroborate the statements which have been made respecting the true and distinct nature of the species and varieties already allotted to the saccharomyces group.

If young and vigorous cells of pure cultures of saccharomyces be introduced into sterilized flasks (preferably assay flasks) containing sterilized wort, and the flask be covered with a double layer of sterilized blotting paper, so that the air can have free access to the interior of the flask, without introducing into it any of its contained germs, it will be noticed that in course of time small flecks or patches of yeast will appear upon the surface of the wort. These flecks, which Hansen asserts have, without doubt, developed from a single cell, will, in course of time, extend, and, uniting, will cover the entire surface of the wort with a film or veil of pure culture, in which the presence of mould or other foreign fungi is impossible. The appearance of this "mothering" in saccharomyces may be hastened or delayed in various ways, but its true appearance is not manifested until the fermentation, with its accompanying frothing, has passed away. Tested by these conditions neither *Torula* nor *S. apiculatus* will form a "mothering." It is found that, as with the formation of ascospores, temperature plays a most important part in the production of the "mothering," and allows of limiting ranges being prescribed for each species. They have been determined by Hansen as follows:

Limiting Temperatures of "Mothering" Formations.

S. CEREVISIÆ I.

Deg. Fahr.		
At 100·4	no formation.	
" 92	flecks feebly developed after	9-18 days.
" 81	" "	7-11 "
" 70	" "	7-10 "
" 57	" "	15-30 "
" 43	" "	2-3 months.
" 41	no formation.	

S. PASTORIANUS I.

Deg. Fahr.		
At 93·2	no formation.	
" 81	flecks feebly developed after	7-10 days.
" 70	" "	8-15 "
" 57	" "	15-30 "
" 43	" "	1-2 months.
" 39	" "	5-6 "
" 36	no formation.	

S. PASTORIANUS II.

Deg. Fahr.		
At 93·2	no formation.	
" 81	flecks feebly developed after	7-10 days.
" 70	" "	8-15 "
" 57	" "	10-25 "
" 43	" "	1-2 months.
" 39	" "	5-6 "
" 36	no formation.	

S. PASTORIANUS III.

Deg. Fahr.		
At 93·2	no formation.	
" 81	flecks feebly developed after	7-10 days.
" 70	" "	9-12 "
" 57	" "	10-20 "
" 43	" "	1-2 months.
" 39	" "	5-6 "
" 36	no formation.	

S. ELLIPSOIDEUS I.

Deg. Fahr.		
At 100·4	no formation.	
" 92	flecks feebly developed after	8-12 days.
" 81	" "	9-16 "
" 70	" "	10-17 "
" 57	" "	15-30 "
" 43	" "	2-3 months.
" 41	no formation.	

S. ELLIPSOIDEUS II.

Deg. Fahr.		
At 104	no formation.	
" 98	flecks feebly developed after	8-12 days.
" 92	" "	3-4 "
" 81	" "	4-5 "
" 70	" "	4-6 "
" 57	" "	8-10 "
" 43	" "	1-2 months.
" 38	" "	5-6 "
" 36	no formation.	

† Lectures before the Society of Arts, London, 1888. From the *Journal* of the Society.

* This is the nearest technical expression to meet the word "voile" used by Hansen.

These observations are of the greatest possible assistance in case of doubt as to the identity of any one species. It will be noticed that the greatest divergences with respect to time are exhibited at the temperature of about 57° F., and in this way it is possible to distinguish *S. pastorianus I.* from *S. pastorianus II.*, whereas by mere reference to their morphology such a distinction would be impossible. The same remark applies to *S. ellipsoideus I.* and *II.*

In general terms it may be stated that those species which sprout at high temperatures require them for their "mothering" formations. It is a significant fact, tending to prove that when fermentation is possible, yeast will not undergo reproduction by endogenous division, that it is scarcely ever possible to form ascospores from "mothering" cells. The worts in which these "motherings" are formed undergoes a peculiar lightening of color which is not noticeable in ordinary fermentations. This is, moreover, typical of true saccharomyces "motherings," and is not shared by other species. The morphological appearance of the various "mothering" cells is truly remarkable, and can best be illustrated by reference to the typical illustrations annexed, presented by Hansen with his memoir upon the subject.

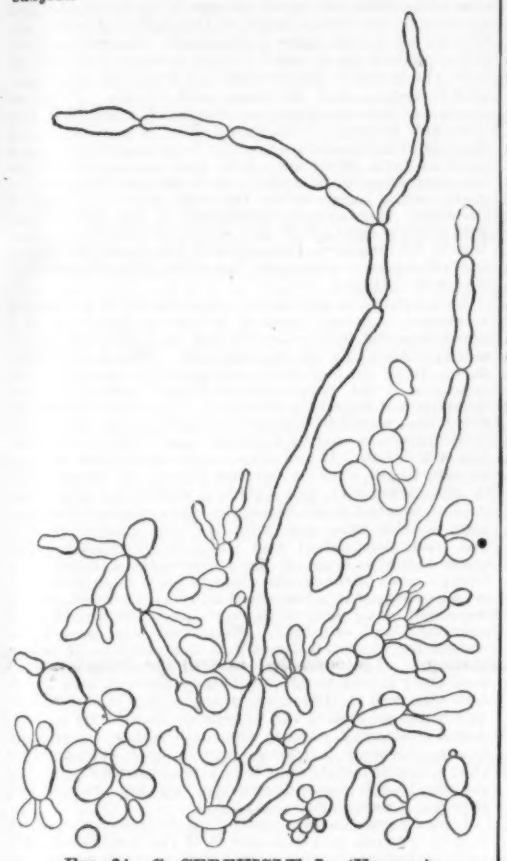


FIG. 24.—S. CEREVISIÆ I. (Hansen.)

It will be noticed that they differ materially from the mother cells which gave rise to them (Figs. 11-16, pp. 10898), and that, although sprouting fungi, they approximate surprisingly in some cases to true hyphal forms. Most remarkable is it, however, that the true type is preserved, and the constancy of the species thereby established, from a totally different point of view to that hitherto approached.

It is obviously impossible, within the limits of these lectures, to devote our attention to the full consideration of those species which are without the bounds of our definition of saccharomyces. Having then defined our true species, and having somewhat minutely investigated their general morphology, it next becomes advisable to examine the conditions upon which their healthy nutriment is dependent.

The cellular structure of plant life was first observed as far back as the year 1667, when it was described with an amazing degree of accuracy by Robert Hooke, one of the first Fellows of the Royal Society. Up to that time the conditions upon which plant life depended for

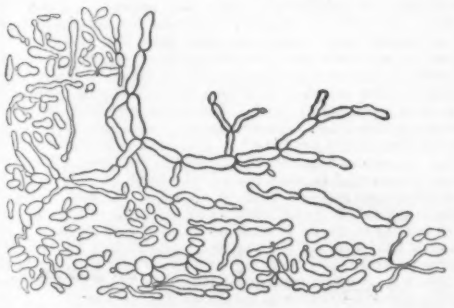


FIG. 25.—S. PASTORIANUS I. (Hansen.)

nutrition were absolutely speculative. Until the various parts of the machinery had been identified, it was obviously impossible to assign to them any specific functions, but once the existence of what Hooke called "cells" was demonstrated, it was not long before explanations were forthcoming as to their mode of action. Indeed, already in the year 1671 it was asserted by the celebrated botanist, Malpighi, that the green leaves of plants contain the essential organs of nutrition. It required many years to substantiate this statement—indeed, nearly a century—for it may be said only to have been finally confirmed by the investigations of Ingenhouss, who, in the year 1779, discovered that the carbon of vegetable cells is derived from the carbonic acid of the atmosphere, and not from the soil. The existence of chlorophyl was next proved, and its necessity within the limits which have already been insisted upon was demonstrated. Then followed the classic researches of De Saussure, who confirmed the work of

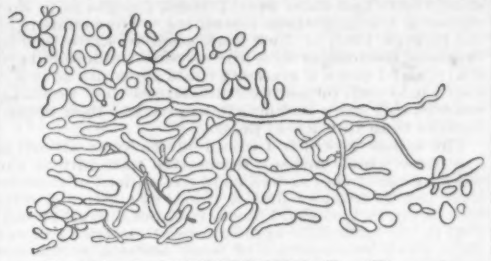


FIG. 26.—S. PASTORIANUS II. (Hansen.)

Ingenhouss, and extended it by proving the true significance of the root system with respect to the mineral nutrition of the plant.

It was at this stage in the development of the science that Boussingault and Liebig attacked the question. It is difficult to assign an exact date to their specific discoveries, because they extended over 36 years—from 1804 to 1840; suffice it to say that they fully confirmed the work of previous investigators, and by its extension laid the foundations of the science of modern agriculture. It was they who first confirmed the fact that plants cannot exist without the supply of mineral elements, that they are derived from the soil, and that as they are absorbed their place must be taken by a fresh supply if the plant is to thrive. If the soil did not contain the necessary ingredients, it was shown that the deficiency could be made good by the presence of certain salts which the plant had the power of decomposing, with a view to the assimilation of the necessary elements. One result of their experiments was the manufacture and use in agriculture of artificial manures.

It should be borne in mind that the works of all these investigators possess more than a transitory interest for us in our attempt to arrive at what should be the nutriment of yeast; because what is true of plant life in the broad signification of the term necessarily holds good for fungi also. Hence it is important for us to know that plants cannot assimilate the elements

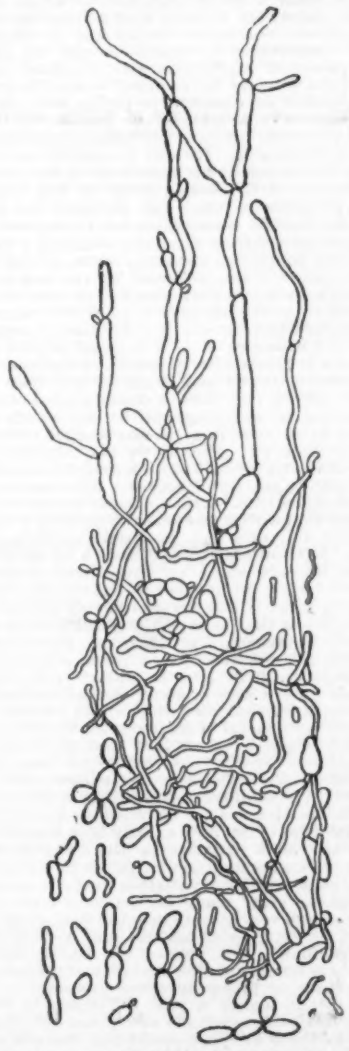


FIG. 27.—S. PASTORIANUS III. (Hansen.)

if presented to their cells in the free state. They cannot assimilate free nitrogen or free carbon, which must be presented to the cells suitably combined with oxygen, and they will then be decomposed by the cell constituents as and when required. If we except the body chlorophyl, which in all probability contains iron, the ultimate composition of phanerogams is indeed qualitatively almost identical with that of fungi.

In both cases we find that comparatively few elements enter into the composition of those complicated bodies which are capable of exerting so remarkable an influence in the economy and life history of plants. These elements are carbon, hydrogen, oxygen, nitrogen, sulphur, phosphorus, lime, magnesia, and potassium. But in the case of plants containing chlorophyl we find one most extraordinary difference. They require a certain amount of iron, *whereas fungi do not require this element at all.* If the iron is present as a mere trace, the plant will thrive; if present in anything be-

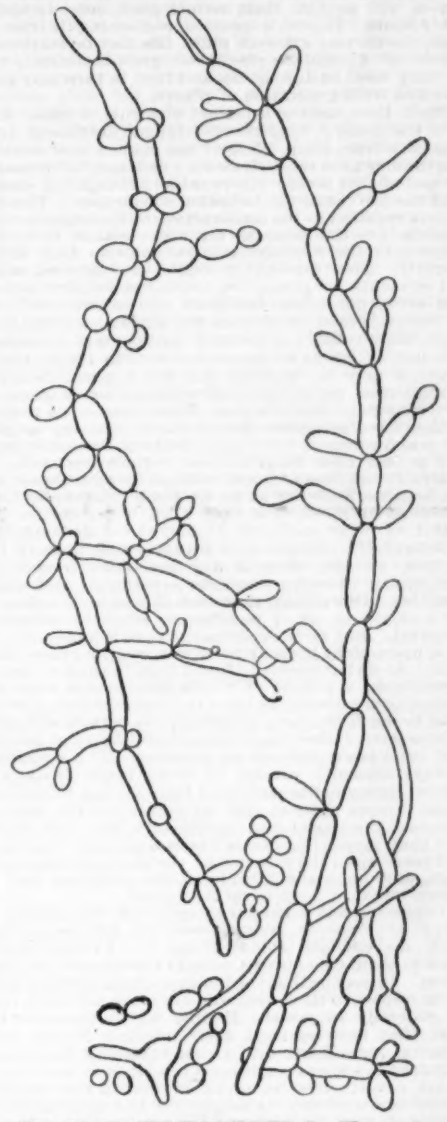


FIG. 28.—S. ELLIPSOIDEUS I. (Hansen.)

yond this it will do harm, and when in excess it constitutes a violent poison to plant life. It has, however, in the following way, been indisputably proved by Gris, and later by Sachs, that this trace of iron is absolutely essential to the production of chlorophyl. Plants may, like fungi, be grown in an artificial nutrient solution. Of course, they require in addition the presence

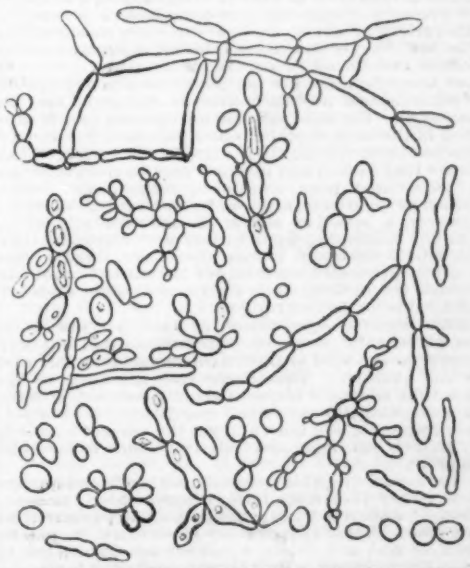


FIG. 29.—S. ELLIPSOIDEUS II. (Hansen.)

of air and light. The solution of Sachs is typical of that best adapted to promote growth:

Water	1,000 c. c.
Potassic nitrate	1 gramme.
Sodic chloride	0·5 "
Calcic sulphate	0·5 "
Magnesic sulphate	0·5 "
Calcic phosphate	0·5 "

But if the solution does not contain a few milli-

grammes to the liter of a solution of iron salt, the leaves of the growing plant will exhibit the disease known as *chlorosis*, will appear white instead of green, will secrete no chlorophyl grains within the protoplasm of the leaf cells, and thus, deprived of the conditions of healthy plant life, will in a short time wither and die. If, however, to such a bleached and withering plant growing in an artificial solution free from iron, the merest trace of iron salt be added, either on the leaves or in the solution, for absorption by the roots, the leaves will acquire their normal green color in about forty hours. In such a solution, containing its trace of iron, the normal cycle of plant life may be traversed. Seeds will germinate, plants will grow therefrom, will develop seeds in due course, and they in turn may give rise to a fresh generation of plants.

Such, then, are the necessary elements of plant life, and the general conditions of fungal nutriment vary but little from them. Nägeli has made a most searching inquiry as to those elements which must of necessity be assimilated before vigorous life in fungi can ensue, and has arrived at the following conclusions: The elements required in the construction of the organic compounds are the same as for other plants, but with respect to the requisite mineral matters they differ slightly. They can exist provided the following mineral elements are present—phosphorus, sulphur, one of the series potassium, rubidium, and cæsium, and one of the series calcium, magnesium, barium, or strontium. Now, since rubidium, caesium, barium, and strontium are not, so far as we know, met with in fungal nutriment, it may be asserted that for mineral elements phosphorus, potassium, and calcium, or magnesium are required. In this they differ from other plants, which have no alternative choice as between magnesium and calcium, but require both at the same time.

The fact that fungi do not require iron, even in traces, for nutriment is not without its significance for us, because, knowing as we do the injurious effect its presence exercises upon beer wort, if present in the water, or other materials employed for mashing, and subsequently brought into contact with hops in the copper, we can, without any hesitation, regard its presence in brewing waters as superfluous and objectionable. Our object, therefore, should be to reduce it to a minimum, or, if possible, to effect its complete removal. But Gris' experiments as to the influence of iron upon plant life may teach us something more than this. As we have seen, he found that, beyond a trace, it constituted a poison. My own experiments upon the influence of phosphates upon the fermentation of worts lead to much the same conclusion—a certain quantity is necessary, indeed no continued healthy yeast growth can take place without its presence. This amount is always normally present in worts made from well grown barley malts—indeed, I have always found that there is more than enough to suffice for the requirements of the yeast; but experiments have also shown me that increasing excess exerts a strongly retarding influence upon the progress of the alcoholic fermentation, and upon the vitality of the yeast—in fact, in every way does more harm than good.

I cannot point to similar experiments with respect to the other requisite mineral elements, sulphur, potassium, and calcium, or magnesium, but I cannot doubt that in their case also an added excess would be injurious. It may be that the limits of quantities advisable with respect to these elements are much narrower than is generally supposed. Making all allowance for the fact that brewers treat their brewing liquors with mineral ingredients with a view to modify the composition of the wort constituents produced in mashing, it must nevertheless be admitted that in this country there is a tendency to exaggerate the mineral treatment of brewery liquors. Within limits it is highly advisable to supply mineral deficiency, but the exaggeration of the treatment is, in my opinion, most objectionable, and cases are within my experience in which it has done much harm.

We have seen that fungi require carbon for assimilation; they cannot obtain it from gaseous carbonic acid, because they are devoid of chlorophyl; they cannot assimilate in the free state, probably because the conditions of plant life require that the assimilable elements should be combined with oxygen, and hence the presence of carbonaceous compounds is necessary. The variety of choice is, however very considerable. The best food is undoubtedly found in sugar, such as glucose and its allied carbohydrates; but, provided that the substances are not poisonous and are soluble in water, almost any carbonaceous compound may be employed. The most suitable nitrogenous food is supplied in the form of proteids and peptones, but it may also be obtained from other sources. Thus Nägeli has shown that carbon and nitrogen may be simultaneously assimilated from *acetamide*, *methylamine*, *ethylamine*, *propylamine*, *asparagin*, and *leucin*. There is, however, a selective action in *oxamide* and *urea*. Though containing both carbon and nitrogen, they can supply the latter, but not the former. Again, cyanogen compounds cannot supply the nitrogen, but can furnish the carbon, while formic and oxalic acids are unsuitable for carbon supply.

This selective appreciation of food is quite as remarkable with plants as with animals, and not only applies to the kind of nutriment assimilated, but also to the quantity. Thus Sachs calls attention to the fact that sea water contains about 3 per cent. of sodic chloride, and only very small quantities of salts of potash, magnesia, and lime, and yet the ashes of seaweeds contain much lime and relatively but little sodic chloride.

The manner in which assimilation is effected renders it necessary that some foreign ingredients, if present, should be absorbed, although they are not required by the plant. Thus elements are often found in plants, such as zinc and silicon, which are now known not to exert any influence in nutritive or vegetative functions. Formerly farmers used to sprinkle sand with their wheat seed when sowing, but the teachings of agricultural chemistry have shown them that such a proceeding is so much money wasted, as the silica is inert.

(To be continued.)

THE art of making stockings in a frame was invented by the Rev. Mr. Lee, in 1589. He is said to have taken the idea from intently watching the movements of his wife's fingers while knitting.

ELECTRICITY, LIGHT, AND HEAT.*

By Prof. C. F. BRACKETT, of Princeton College.

GENTLEMEN: This is the first time in my life that I ever had the pleasure of facing an electric club, so far as I remember, although it has been my fortune, good or otherwise, to give instruction in electricity, and I am bound here to confess—and it is a very willing confession—that if I had students under such auspices as I should have had them here, I think I might have succeeded in teaching them something. The members of the Electric Club of New York seem to know how to surround themselves with the physical inducements to study, and I think if students could generally be introduced into such pleasant circumstances they would be considerably more inclined to appreciate their opportunities than they are at present.

The topics to which I would invite your attention are those which present themselves naturally to the working electrician as well as to the student of electricity. Let us consider first a practical matter. I refer now to the fact, so well known, I suppose, to all of you, that in the immediate applications of electricity to our daily wants we make use of those material substances called conductors, and that we interpose them in the path where we would like electricity to flow, but where we do not wish it to do so, we place substances called non-conductors. It is known to you all that there is no very sharp line of demarkation between these two kinds of bodies. It will suffice in general to say that the common commercial metals act very well as conductors. I desire to draw your attention to two points, and, for the purpose of illustration without a blackboard, I will ask you to follow me with your imaginations. Suppose one point A, and another point B. They are at different potentials. If we join the point A with the point B by means of some metallic conductor, we shall find that in a given time, very short, there will be no difference of potential between A and B, and we shall find also, if we use proper appliances, that something has been going on in the wire, or in the region of the wire, which connects those two points. When we say "in the wire," we are not sure that we are telling the truth; when we say "in the wire, or in the region about the wire itself," we are sure. If we employ successively copper, gold, or silver wire, we shall find that the equalizing of the electrical conditions of the points A and B is in general not accomplished with equal facility. In other words, we find that all bodies are not equally good conductors. Now, copper wire being generally used on account of its excellence as a conductor and its moderate cost, it interests the electrician to know when he has the most efficient variety of copper wires.

The matter then which I wish to discuss with you first—and let me say in passing that if I do not make every point clear, I invite questions as we go on—is: How is the electrician to know that he is using the very best quality of conductor under any given circumstances? Oftentimes he wants to use the greatest possible power of conduction with the least possible bulk. Again, he may be required to use the greatest possible amount of conducting power with the least possible amount of weight, as for instance in the construction of certain pieces of electrical apparatus, such as the dynamometer. He will be guided in his constructions very much by his knowledge of the materials, and especially of the conductivity of the materials, which he proposes to use. Let us speak for one moment of the specific conductivity of conductors. The word is used precisely as in science we use it, with reference to other properties of matter when we refer them to a standard. It was assumed by the committee of the British Association that *pure* metals should in some way be taken as the standards to which commercial samples should be referred. In the case of copper we must be able to secure it in a state of perfect purity, and to draw it into a wire of perfectly uniform diameter. A definite length and diameter will then fix the standard. Or we may take a definite mass of copper, as determined by weighing, and fix the length of the conductor to be made from it, and assume that nature will fix the diameter. Finally we may attempt to deduce the diameter of a known length from its weight and the specific gravity of copper. All these plans are beset with difficulties. Let us discuss the matter.

If we say that a wire one one-thousandth of an inch in diameter and one foot in length shall have a resistance of 9·718 ohms, we have made a statement equivalent to the one made by the British Association committee. How can you know that this wire is a foot in length? That is not a very difficult matter. How are you to know that it is a thousandth of an inch in diameter? There comes a difficulty. But the British Association did not attempt to make a wire just one one-thousandth of an inch in diameter. On the contrary, they drew a wire of some size and deduced from the resistance of that wire what would be the resistance of the same metal if its diameter were reduced to one one-thousandth of an inch and its length were made one foot. Now let us, before we leave this, inquire what are the difficulties in the case. We have a wire which in general terms we will say is one-sixteenth of an inch. Matthiessen's. We want to know in terms of thousandths of an inch exactly how much it measures, for the least variation in the diameter of it of course implies a much larger variation in the area, and that implies a very large variation in the conductivity, so the slightest error made in the measurement of its diameter enters very largely into the final result. It is true that we have screw gauges and vernier calipers at our command; but yet we can hardly by any of these devices determine the real diameter to within at least fourteen or fifteen hundred-thousandths of an inch, and that makes a vast difference in the conductivity of a wire, especially in the case of small wires. Let us take the other plan, specifying that the samples being a foot in length shall weigh so many grammes or grains, and then again we will have one of the British Association methods on the basis that a cubic foot of copper weighs 555 pounds—that it is nearly 555 pounds; if a cubic foot weighs about that, and not exactly that, there is a doubt about it, which will seriously affect the result. The difficulty is at once seen when stated. They also specified that the specific gravity shall be so and so, or that it shall be so and so. Now, if we take the Smithsonian tables collected under the direction of Professor Henry, we shall find that the specimens of copper there reported, as respects their specific gravity, vary all the way from about 8·95 down to 8·40, a variation enough to make a very great percentage difference in the conductivity of copper. Now to what are those discrepancies due? The question what is the specific gravity of a metal is the one which can only be determined in a given case by taking that particular piece of metal which is to be employed and determining its specific gravity. Then let us look at the process of wire drawing. Suppose we draw the wire through the draw plate. What is the operation that goes on? Is the wire simply stretched and made longer and smaller? Is the wire compressed as it passes through? The experience of the wire drawer will perhaps come to our aid and tell us something about it. I had occasion some time ago to apply to one of the most expert drawers in the great works at Trenton—Roebling's—using a jeweled draw plate through which the wire was to be drawn and having a finished sample of wire to be drawn through it. The workman at once said: "That cannot be drawn through it." Said I, "Why?" He said, "The wire is too small." Now there may be and there probably are gentlemen present who will see at once the reason of that statement. It is found that if we wish to reduce a wire from one size to another, we must reduce it by a considerable amount or we cannot draw it through a plate at all. The wire is a little large for the hole. Instead of there being pushed up a mass of the metal in front of the hole, the metal is compressed and becomes very hard, and is wedged into the plate, and breaks. If, on the contrary, the reduction in size is so great that this does not happen, it is found that it can be drawn through with moderate tractile force and that it has a more uniform structure. Let us then consider what happens when we attempt to pull the wire through the plate, what happens to the wire after it is pulled through. Here experiment comes to our aid. If you will pardon me, I will use a pencil for illustration. Wertheim, in some experiments of his, found that when any substance is stretched, the entire bulk of that substance is enlarged.

An instructive laboratory experiment is performed by means of a long mass of rubber in the form of a parallelopiped, whose length can be measured, whose side and thickness can be measured. When we stretch it, we find that if it be calipered at uniform distances, and the volume under those circumstances be computed, it is greatly increased. One would naturally think that would be diminished in section as much as it is elongated, so that its volume would remain constant; but it is not so. If you take a piece of rubber as long as your finger, find its specific gravity by weighing it in the usual way, put this in a stretching apparatus similar to what is used to make one's pants cease bagging at the knee, you will find its specific gravity has been diminished and the bulk increased. The same thing is true of the copper; the wire which is being drawn through the plate is kept back constantly, a portion of it is here used as spare material, as if it were still being pushed through, and after that it is stretched and enlarged. Therefore the least bulk, weight for weight, is directly in the plate as it passes through it. Now the question which arises here immediately is how much it will be expanded, and therefore what will be its specific gravity after it is through. You must see that it will depend on how much it is reduced in size and what is the condition as regards annealing before it is drawn through and afterward, so that when you state that wire is hard drawn and has such and such a specific gravity, you are stating two things not necessarily compatible.

If you have a certain mass of matter having a certain length between points A and B, if the mass be highly compressed so as to occupy only a small space, one might suppose that the particles are nearer together, and therefore they could less readily execute the motion involved in the transfer of electricity from one point to another. One might say, on the other hand, if it were annealed, rendered larger, there would be greater freedom of motion and so greater conducting power. Those are both theoretical matters, and there are experiments tending to prove the truth of either position. It is found that the conductivity is improved by being annealed. But what do you mean by annealing? Do you mean the wire shall go into the oven and be heated to redness and kept so a certain length of time and be then allowed to cool spontaneously? A short time ago a specimen of wire came to my notice to be measured for conductivity. It was found to be very low. On examining its physical properties it was found that if it was flexed sharply on itself, any attempt to straighten it back broke it short off.

The question was given to me for investigation, to find to what this was due. The conductivity being too low, and on finding the physical state of it to be such as we would call brittle, it was supposed, of course, that it might contain arsenic, or that it might contain sulphur, and very rigid tests were made for these substances, but neither of them was present. Tests were made for oxide of copper, which in the annealing process might have been absorbed into the interior. On making a careful test for suboxide of copper there was found to be no more present in it than in other wires which were tough and whose percentage of conductivity was over a hundred. Then the question arose, To what is this due? Accordingly, a sample of the tough wire that could be flexed in any way you pleased, that had a percentage of conductivity of over a hundred, was taken and heated for a little time in the presence of hydrogen gas—heated to redness. It was found on cooling that it would not bear a single sharp flexure. On trying to make it bend as an ordinary wire, it would break half way off. It appears then that by the presence of hydrogen, and the existence of a high temperature, a new molecular condition was impressed upon it. This investigation was made by Dr. McCoy.

Now, along the same line I might note another experience which was accidentally come upon. A large amount of iron wire was made for telegraphic purposes. It was shipped to its destination, and on being unloaded, roughly as usual, the wire was found to break into pieces as if it had been a rod of glass. On being carried back to the factory, and nothing else done to it except to draw it through the draw plate, it was thoroughly restored, showing that the whole matter is due simply to molecular change—that it was restored by mechanical treatment. Now this short discussion will settle the point which I have brought up—what did the British Association mean when they said a wire so long

* An address before the New York Electric Club, December 20, 1888.

having such a diameter shall be taken as a standard? They meant absolutely nothing, for they have not specified how it is to be drawn through the plate; they have not specified how it is to be annealed or how many times it is to be annealed, or any specific mechanical detail about it.

Now, let us consider another phase of wire drawing for a moment. Suppose we had a standard. Suppose these questions were all settled, first as to the purity of the material, second as to the method in which it should be drawn, and how it should be annealed, and what kind of draw plates shall you draw it through. If you are to use jewels, such as a topaz or something of that sort, for every final drawing of the wire, that course would seriously embarrass every concern in the community. You can draw only a very limited amount of wire through the plate, and then it is worn out. We must, therefore, fall back on steel, the only available thing, and see how that will work. I will narrate a little experience. Having wires sent to me by various firms to study and report, I have invariably found that when a lot of wire which was continuous without any joints was examined, both ends of the wire were large and the middle small. I think that is probably the experience of all—that the middle is reduced. What, therefore, is the diameter of the wire?—how shall we get it? It is larger in one diameter than the one at right angles to it, and gives you neither a circle, an ellipse, nor any other regular geometrical figure. Well, of course, those who wish to save trouble say, let us weigh it. Suppose we determine to measure it without weighing it. Of course we will have to run our calipers around the wire, and we have to discuss the figures we shall get from that, and having determined the area of the cross section and the length of the wire, we can refer it to the standard of the British Association, and if that were accurate, our work would be done. But we find that it is not safe to assume that these wires will be uniformly smaller in the middle, and we have therefore to look into the facts.

When we begin drawing the wire through the plate, the first effect of it is to heat the plate. Before the heat is conducted to the outer parts of the plate the interior parts get warm, and they have no other way of expanding except inward, thus contracting the hole. The consequence will be that, for some time, the wire will pass through the hole at nearly its full diameter; but from the beginning the heat evolved in consequence of friction will cause the diameter of the hole to decrease until at length a point will be reached when the conduction and radiation from the plate will remove the heat as rapidly as it is produced. Meanwhile the rough surface of the wire will cut away the plate and thereby increase the diameter. There will thus be two stages in the alteration of the draw plate; during the first the diameter will on the whole be diminishing, and during the second it will be increasing. Accordingly the wire will be smaller at some point between the extremities than at either of them.

The wire will, then, be of varying diameter, and one who would ascertain it for the purpose of finding the conductivity of the sample will have to make a series of measurements at various points along the length as well as in various azimuths about its axis, very much as the surveyor treats an irregular piece of land. Suppose he wishes to avoid all the labor and says, I will weigh it. There is difficulty if you do weigh it; how will you deduce the bulk from the weight without knowing the specific gravity? If you turn to tables, you will find twenty or thirty different specific gravities assigned to copper wire. If you say, I will determine the specific gravity myself, then the expense will be too great if any considerable number of samples are to be tested.

Now, having stated some of the difficulties with reference to the matter, let us inquire what ought to be done. It is known, I suppose, to most of the gentlemen present, that the British Association committee still have this matter in charge, and have undertaken at their recent meeting at Bath to have the whole question examined, and it is to be hoped that something will be elicited which will give us a standard for the conductivity of copper wire, so that we shall know whether we are getting one thing or another. It may surprise some of you to be told that we very frequently have from the manufacturers in this country samples which will run as high as 103 or 104 per cent. of conductivity. In other words they are three or four per cent. better than they can be, taking the British Association as a standard. Now the matters which I have already brought forward of course enter into the reasons. I stated at the outset it was assumed the pure metals were to be accepted as standards. That is not the whole matter. It does not by any means follow that when you have assured the purity of a metal you have secured the highest conductivity. You have also to ask what is the length? what is the diameter? what were the methods of its preparation? and unless they are all defined, there can be no certainty that you are getting within two or three or three or four per cent. of the proper conductivity.

But some one says: What is the use of all this after all? There are conditions in the art of the practical electrician in which he must say, beforehand, precisely how many feet of wire he can employ, precisely how much space they must build up in order that he may have symmetry, equality, and proper conducting power. I think any one who has attempted to wind the coils of the dynamo machine so as to secure a definite number of ampere turns will agree that a variation of two or three per cent. in the conductivity of hic copper wire makes the difference between success and failure.

Now, having said so much with reference to a practical problem that interests everybody, let us turn our attention a few minutes to matters that are more speculative. It is not only true that it becomes important for us to have conductors which shall convey from one point to another the disturbances which we call electrical; it is also true that in the construction of machinery at the present time we are to take into account the air spaces between those conductors; we have to take into account the intermediate thing which separates one conductor from another, and inquire what goes on in that space. And it will not astonish many present when I put it this way—theoretical electricians who are studying the question very hard are getting to look at it in this way: We are not more concerned with what goes on *in* the conductor than we are with what goes on *about* the conductor. In other words, the principal transaction in the electrical transmission of energy from one place to another goes on, not in the substance of the wire, but in the space which surrounds the wire. We ought to get into the spirit of these electricians, and look at the ether through which we move as the most important of all materials with which we have to deal. Think now of the air and of everything utterly vanished, a perfect vacuum, and then cut out a solid "chunk" of the vacuum and see how it behaves. We shall find probably in the light of these investigations that this is the most important thing we have to deal with, that the whole action which we call transmitting goes on in that, and that the conducting wire is only a hole bored through it. It is simply so many masses of matter called molecules that are swinging around and making an easier path, and this determines the direction through which the electrical disturbances shall be manifested. There are certain transactions which we may look at. For instance, a wire can act inductively through a vacuum just as well as through the air. No one is, I think, able to imagine to himself, under any circumstances, whatever he may say, action across a space where nothing exists. Faraday, in grappling with this same subject, said, in substance, that it was unthinkable. Newton gave his great deliverance also along the same line.

If, then, action at a distance must be ruled out, there is something between the conductors themselves which we have to take into account. Now, the experiments and researches to which your attention has been called in the electrical literature of the day, which have been going on for several years past—in fact, they date back many years—have cast a great deal of light on this subject, and while it may not seem immediately practical, it may be that some word which will be dropped in what I shall say, or in the discussion, will set some one to thinking.

In order to proceed step by step, let me propound the ultimate question which I suppose may interest a large number—how to produce an electric light in the very cheapest possible way. That is a practical question. Now I am going to try to see if we can get to it in a philosophical way (without realizing it), but upon the paths, if possible, by which it will probably be ultimately reached. Faraday, in one of his brilliant papers in which he set forth the principles of "the new electrical machine"—the starting point of modern dynamo appliances—made use of words like these:

"I have rather, however, been desirous of discovering new facts and new relations dependent on magneto-electric induction than of exalting the force of those already obtained; being assured that the latter would find their full development hereafter."

Now, in the light of what Faraday said, let us look at this subject. Nothing will be practical that I shall say. I shall not show you how to make that machine, but I propose to scale past the lines along which it may come, and in doing so I will be very brief. First, what is light? This solid ether which we have spoken of, which is a necessity in the phenomena of light, and which Fresnel assumed as the existence in order to tie together the inevitable deductions from his equations, must be thought of as something evidently more solid than the solidest things we think we know about. When we get light we get it by setting up vibrations in the ether, and all I say about these vibrations is this: they are periodic, they are uniform, they are transverse to the line of progress, so that if the beam of light were going directly from me to you, the motion is athwart the beam, so that a particle can be thought of as moving this way and another round that way (illustrating with pencil), and so in every possible direction at right angles to the line from me to you about that. Now the question is, is there anything connected with electrical science by which we shall be able directly, ultimately, to evoke that method of movement?

In 1832 Professor Henry showed that when a Leyden jar is discharged, the spark which we have been perhaps accustomed to think of simply as a passage of electrical fluid from one side to another, on the contrary, was a series of transmissions back and forth from the outside to the inside. This he showed to his own satisfaction, and embodied his conclusions in a paper, which is, of course, extant at the present time. Later, Fedderson took up the question and studied it with care, and showed how to compute the length of time involved in any one of the elementary discharges passing back and forth.

In 1855 Sir William Thomson showed how to compute, from the induction of the wire and the capacity of the jar, the time interval involved in one of those oscillations. Here is a trough of water on one end of a pipe and another on the other end. The one trough being empty, the water runs through and accumulates on one side, and by its impetus it will pile up a little higher, and we will have a series of oscillations, and finally it will come to rest. That is exactly what happens when a Leyden jar is discharged by a series of oscillations. Now we can put such a conductor between the two troughs that there shall be only a steady flow from one side to the other. We shall find that there will be no oscillation in that case back and forth. If we put there a very small wire or a wire having large inductive capacity, I can get a steady discharge in the case of the Leyden jar. Any way we can control it and show what is the length of time involved without any difficulty.

(To be continued.)

PHENOMENA PRODUCED BY ELECTRIC DISCHARGES ON EASTMAN'S PELLICULAR PAPER.

By M. E. L. TROUVELOT.

IF we effect a discharge from the positive pole of an induction coil upon the sensitized surface of a sheet of Eastman's negative paper, we perceive the ramifications of the sparks designed in marks of fire upon this surface, and on developing we obtain a bold image comparable to that obtained by the same process on glass plates. If we make the same experiment with the negative pole, the results are different; we recognize then only some short ramifications upon the pellicle. If we raise up slightly the corner of the pellicle, we observe that the line of fire has traversed the pellicle and the paper, and continued its course to the surface of the plate of glass supporting the condenser on its opposite side. On developing, the marks perceived on the pellicle appear rapidly, and it is perceived that they are considerably thickened at certain points, and much resemble certain mosses which grow upon the branches of trees. These mossy portions often terminate in the form of a club at the very point where the luminous line traverses the pellicle, as if their formation was due to the resistance offered by this pellicle to the passage of the spark. If we carry out the development of such a proof, we see traces, more or less distinct, of the ramifications which have been produced under the pellicle which is found impressed on its lower surface through the paper.

If we take a proof thus obtained when it is fixed and well dried, and after placing it on the condenser we cause a discharge of the coil to take place upon the corresponding point where the first discharge has taken place, we observe a singular phenomenon. If we discharge the *negative* pole upon a proof which gives an image of *positive* electricity, we remark from the first discharge faint whitish lines, which lengthen and widen gradually after other discharges which succeed to the first; these whitish lines, which look as if finely drawn with chalk, follow exactly the ramifications of the positive proof in all its branches. If we make a similar experiment by discharging the positive pole upon an image of the negative electricity, nothing similar appears, whatever is the number of the discharges produced.

We often see the images given by the discharges of the positive and negative poles intermixed in the same proof. If we direct discharges of the negative pole upon the same proof, we see gradually the positive ramifications appearing white, while negative ramifications remain unaltered, except where these ramifications are accompanied by positive lines following the same track. Similar experiments made with proofs on glass give no result.

If examined with the microscope, these whitish lines seem to result from thousands of minute blisters of the pellicle, doubtless occasioned by the rise of temperature due to the passage of electricity over the same lines.

These experiments show anew the difference of the characters of the opposite electricities, and prove that negative electricity possesses a power of penetration far greater than that of positive electricity.—*Comptes Rendus.*

IMPROVED ACCUMULATOR.

By A. RECKENZAUN.

MESSRS. COMMELIN, Desmazures, and Bailhache in May, 1887, brought out a new accumulator, in which copper plays an important part. For the description of this we are indebted to M. Reynier's work, "L'Accumulateur voltaique."

The positive electrodes of this battery are made of porous plates, obtained from finely divided copper under a pressure of 600 kilogrammes per square centimeter. Electrolytically reduced copper, free from oxides, is used.

The negative electrodes are of tinned iron plates; these are amalgamated. The tin has to hold the mercury, which does not adhere to the bare iron. The receptacle is made of tinned sheet steel; the negative electrode rests at the bottom of the box, and it is in contact with it. The electrolyte consists of water, zinc, and potash in the following proportions:

Water	1,000
Zinc	144·67
Potash in solution	209·82
" free	313·72

The positive electrodes are enveloped in parchment paper, and insulated from the negatives by means of glass rods. The parchment is stitched on, and although the seam is not water tight at first, it becomes so through the action of the liquid, which causes the paper to stick fast. The parchment undergoes a change through the action of the alkali; it swells and becomes brittle, but it appears to hold on a considerable time without breaking, if no mechanical disturbance takes place.

Without the parchment the action is irregular; the oxidation of the positive electrode is not complete, and deposits of zinc are mixed with copper; hence want of adherence and local actions or short circuits through the formation of "trees," as in other batteries where metallic deposition takes place. These envelopes, therefore, play a very important part.

M. Finot, the chemist who examined this invention, gives the reactions during the discharge thus:

$Zn + KO, HO, Aq + Cu_2O = Zn\,O, KO, Aq + 2\,Cu + HO$,

but there appears to be a doubt as to whether this is the correct rendering of the equation, and it requires confirmation; curiously, there are hardly two authorities who agree on the subject of chemical reactions in secondary batteries.

M. Finot ventures another theory in the *Bulletin de la Societe Internationale d'Electriciens*, where he states that during the charging of a copper accumulator one observes a curious phenomenon, which tends to show that oxygen does not combine with the copper, but that it may be occluded, and this occlusion may be due to peculiar properties of the porous plates.

When the apparatus is being charged one may observe that from the commencement of charging the level of the liquid rises and continues to rise until the charge is discontinued. On the other hand, during the discharge the reverse takes place; the liquid falls gradually, and does not resume its original level until the battery is entirely discharged. If, says M. Finot, the oxygen did combine with the copper, no rise in the level of the liquid would manifest itself until all the copper had entered into combination. Thus the oxygen fills the pores of the plates, displaces the liquid, and causes the increase in volume.

During the discharge, again, oxygen should disappear immediately, and the liquid resume its normal level a few minutes after closing the circuit, but this is not actually the case.

M. Reynier thinks that these observations do not appear sufficient to demonstrate that occlusion of oxygen takes place in the positive plates, and he asks whether the rise of level in the liquid is not due to changes of volume in the plates, one of which is oxidized and the other covered with zinc? In order to decide this question, the densities of the liquid as well as of the positive plates should be ascertained after the charge; from this may be deduced the total volume of the elec-

trodes and of the liquid. By this method it could easily be determined whether the hypothesis regarding the occlusion of oxygen is correct.

The following weights and dimensions were given by M. Commelin, and they refer to an accumulator which has been constructed for the purpose of propelling a small torpedo boat. The storage capacity of the cell has been ascertained in the laboratory by the inventors:

Weight of cell in working order	22·046	lb.
" " 5 positive plates	4·25	"
" " 6 negative "	2·32	"
Height " positive "	11·02	inches.
Width " " "	4·92	"
Height " negative "	11·81	"
Width " " "	4·92	"
Surface of positive "	54·23	square inches.
" " negative "	58·1	" "
Receptacle, length	5·905	inches.
" width	3·346	"
" height	15·75	"
" weight	2·204	lb.
Electrolyte (1·55 S. G.) weight	13·224	"
Useful E. M. F	0·75	volt.
Current, charging	15	amperes.
" discharging	48	"
Time of charging	30	hours.
" " discharging	9½	"
Useful capacity	413	ampere hours.
" "	0·42	h. p. hour.
Capacity per pound of cell	18·72	ampere hours.
" " " "	14·12	watt hours.
Weight per h. p. hour	52·47	lb.

According to a note (communicated in March, 1888) by M. Krebs to the Academie des Sciences on "L'Essai d'un moteur électrique alimente par des accumulateur destinés à un bateau sous-marin," the total weight of a Commelin-Desmazures battery to give 1 h. p. hour was found to be 37 kilogrammes, or 81·55 pounds.

This is not a very great advance upon what has been accomplished with certain lead storage batteries, weight for weight. The volume or dimensions of the copper storage battery are smaller than those made of lead, while the efficiency may be alike in both.

With regard to cost, one may infer that lead and dilute sulphuric acid must be cheaper than copper and potash; and as to other points of interest, we have to ask whether it is necessary to amalgamate the negative plates frequently? How long will the parchment envelope last? What changes take place with the electrolyte in the course of time? No doubt positive plates made of copper are more durable than those made of lead, but the general item, *depreciation*, has yet to be ascertained; if it turns out to be a very small quantity, then we may ignore the question of first cost, and then we can congratulate Messrs. Commelin, Desmazures, and Bailhache, as well as those in search of a novel storage battery, for an achievement the beneficial results of which cannot easily be overestimated.

THE PLANETS FOR FEBRUARY, 1889.

By "ROYAL HILL."

WHEN, as on the occasion of some interesting occurrence in the heavens, such as the appearance of an unusually brilliant planet, or the close conjunction of two or more of our neighbors in the solar system, we inquire, What are the actual positions of these bodies at this time? which is the nearer? in what directions are they moving? and what is the reason of their near approach to each other, or the cause of this unwonted splendor? we find that up to the present time there have been no means available to the non-professional reader by which we may satisfy our curiosity on these points. The almanacs merely record the fact of these phenomena, and the records of the planetary movements usually published in scientific journals give us no key to an understanding of them.

As we watch the planets in their ceaseless wanderings over the sky, now appearing as evening, now as morning stars, changing in brilliancy as they approach or recede from the earth, crossing and recrossing each other's paths, one time moving in their regular eastward march, and then moving backward with an apparent motion which we know does not really exist, the interest that attaches to these observations would be greatly enhanced if we had some key to them, and could understand the causes of the phenomena presented to our vision.

Such a key is presented, it is believed for the first time in the history of astronomy, upon this page to the readers of the SCIENTIFIC AMERICAN SUPPLEMENT, in a map of the solar system, drawn in all respects accurately to scale, and showing the positions of the major planets on the 1st of February, 1889, and the amount of their motions during the month.

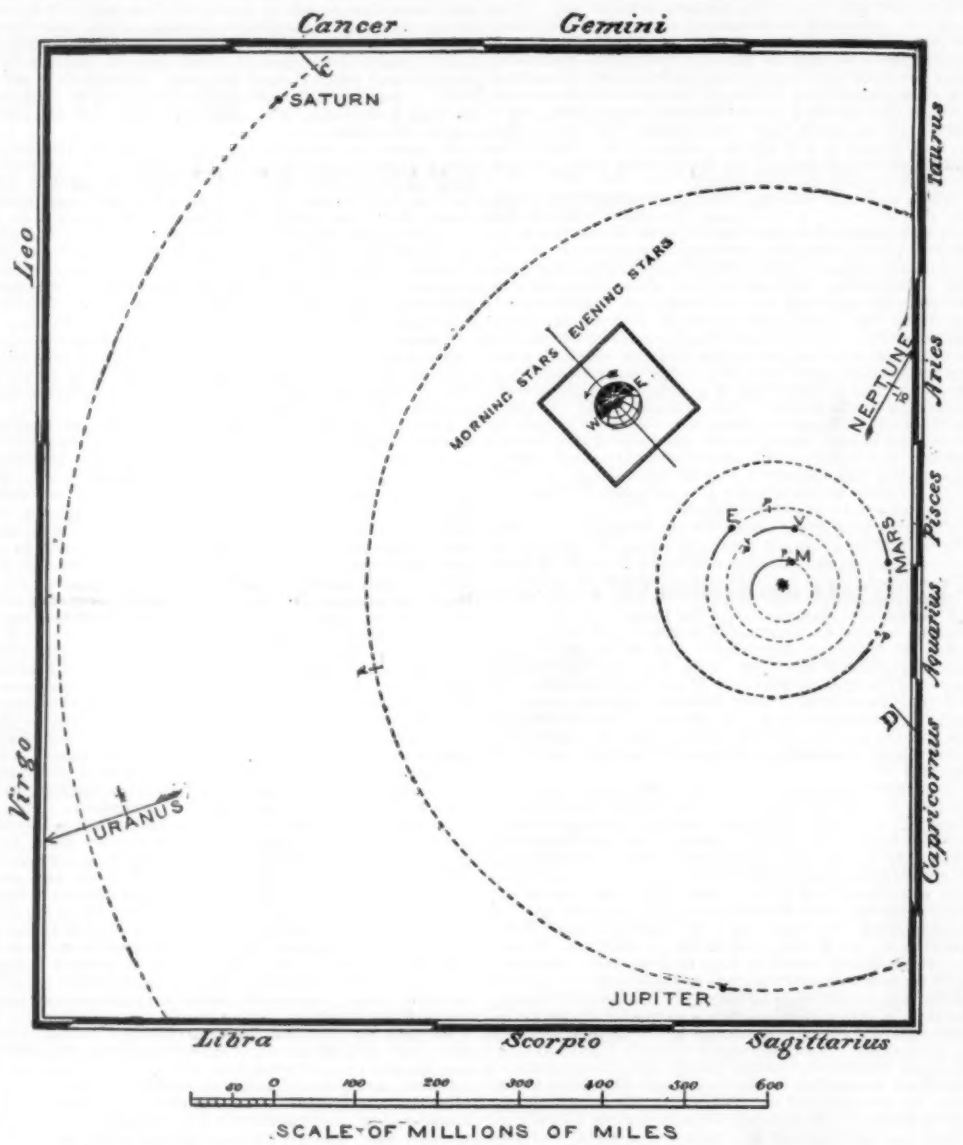
As the orbits of the planets are here shown in their proper proportion to each other, their longest axes and points of nearest approach to and farthest distance from the sun, and their eccentricities to that body, all at their proper angles, the map becomes an illustration to the almanac for the month of February for this year, and presents at one view every conjunction, opposition, elongation, or other phase of planetary movement, the most of which are represented in almanacs by cabalistic signs.

But the map will show much more than this. Being drawn to exact, though necessarily minute scale, the ever varying distances of the planets from the sun, the earth, and each other, at any period of the month, may be measured with a fair degree of precision. It also shows which planets are morning and which evening stars, and the reason they are so is made apparent at a glance. Each of the planets may also be located with reference to the stars by means of the zodiac constellations, from which the major planets never deviate, and which are marked around the edge of the map. Strangely enough, also, this little map is a complete index to the positions of the planets in the sky at any time of the night or day, even if they are not above the horizon or the reader unable otherwise to identify them. The manner in which the map is used for this purpose will be shown farther on.

In order to turn our map to proper use, let us examine the positions of each of the planets for the 1st of February, as marked by the dots upon their respective orbits. Let it be noted here that these dots represent the *positions* of the planets, not the bodies themselves, for no dot could be made on paper small enough to represent even the giant bulk of the sun upon the scale to which this map is drawn.

It will be noticed that part of each orbit is drawn with a full line, the rest being dotted. The full line shows how much of the orbit is covered by the planet's movement during the month, the dot representing its position on the 1st day of February and the end of the full line its position on the 1st day of March.

Let us first notice the position of the earth, which is marked by the letter E. The letter *p* on the earth's orbit shows the point where the earth approaches nearest to the sun, which point is passed by the earth on the 31st of December. We were at that time 90,837,000 miles from the sun, but have increased our distance, so that we are now 91,064,000 miles distant from him, and this distance may be verified by measurement on the scale. A line is drawn through the sun and the earth, and continued to the edges of the map at C and D. This line divides the morning and evening stars. The little diagram on the line explains this. There the earth is represented in plan and as turning from west to east, as shown by the arrow. As the observer passes from day to night at E, the sun disappears, but the planets Mercury M, Venus V, Mars, and Neptune remain above the horizon, and are therefore still visible as evening stars, until the revolution of the earth carries them also out of view. As the night wears on, Saturn will be brought into view, then Uranus, and lastly, just before sunrise, Jupiter will make his appearance, till the sun rises on the observer as he passes the line at W, and extinguishes all lesser lights in the blaze of day.



POSITION OF THE PLANETS FOR FEBRUARY, 1889.

Next turning our attention to Mercury, marked M on his little orbit, we notice that he is very near to the point on his orbit marked *p* (for perihelion), when his distance from the sun is at the least, about 28½ millions of miles, as may be measured on the scale. At the opposite side of the sun, we see he will be much farther than this, the scale showing the distance there to be about 43 millions. Another thing we note about the position of Mercury is that he is near his greatest elongation from the sun, as seen from the earth, for a line drawn from the earth to Mercury is nearly tangent to his orbit. This means that he is visible in the west after sunset, and a careful examination of the western sky about three quarters of an hour after sunset will enable the observer to pick out this sparkling little planet, which is so difficult to find that some people pass their whole lives without once catching a glimpse of him.

By measurement on the scale it will also be noticed that Mercury is now about 83 millions of miles from us, but as he swings in between us and the sun he lessens this distance very rapidly. He is in conjunction with the sun at 8 o'clock on the evening of the 14th, and then becomes morning star, setting before and rising after the sun. It is not until the 17th that he is at his least distance from us, being only 59 million miles distant. He then begins to move away, and on the 28th his distance is 67½ millions from us and 39 millions from the sun, as we may measure by the scale at the bottom of the illustration.

Venus, which now shines with such splendor in the western sky after sunset, is the next to claim our attention. The dot marked V shows her position on her orbit, which is more nearly a circle than any in the solar system. In fact, it is not possible, on a map of this size, to show the orbit of Venus as any other than a circle with the sun as its center—a condition strikingly different from that of Mercury, which, as will be noticed on the map, is very eccentric indeed. Venus, on the 1st, is about 66½ millions of miles from the sun, and does not vary very much from this during the month or indeed at any time. Its position with respect to the earth, however, varies greatly. On the 1st Venus is distant from us 76,400,000 miles, and the map shows that it is coming almost head on toward the earth during the whole month, on the 28th the distance being reduced to 57,820,000 miles, an average approach of 740,000 miles a day and nearly 31,000 miles an hour. On the 17th of the month Venus reaches her greatest elongation from the sun, that is, she swings out from the sun to her greatest distance, as viewed from the earth, a line drawn from the earth to Venus on that day being tangent to her orbit, as may be verified on the map by estimating the positions of the two bodies on that day and maneuvering a little with a fine thread or the edge of a piece of paper. Brilliant as Venus is now, it will increase in splendor as the month wears on.

The orbit of Mars lying outside to that of the earth is large enough to allow the planet to be marked with its full name. Mars has passed its perihelion (marked *p*) or point of nearest approach to the sun. His orbit, as will be seen by the map, is very eccentric, as the reader may measure for himself. His distance from the sun on the 1st is 129,900,000 miles, on the 28th 132,350,000, and from the earth 193¼ millions on the 1st and on the 28th 205,440,000 millions, so that while Mercury and Venus approach us during the month, Mars recedes. All of this is very apparent on examination of the map, and the distances may be measured quite nearly on the scale. During the month of January Mars has appeared very close to Venus. The reason of this appears on the map, for a line drawn through the earth and Mars passes quite close to Venus, though Mars is so far off that he is at present quite inconspicuous.

Jupiter, which is the next planet from the sun, has an orbit so large that only part of his immense circuit can be shown on the limits of this page. The farther a planet is from the sun, the slower it travels in its journey, and the full line showing the course of Jupiter for the month is therefore very short. On the 1st he is 487,840,000 miles from the sun, and on the 28th 487,120,000. The point of Jupiter's orbit nearest the sun is not shown on the map, but the point of farthest distance or aphelion is marked with a small a. He

passed this point on the 23d of August, 1886, at which time he was 504 millions of miles from the sun. His distance from the earth on the 1st of February is 549,660,000 of miles, and as our motion is toward him, this distance is decreased to 516,600,000 miles on the 28th. Jupiter is on the western side of the sun, therefore rises before him, and is morning star, and a very conspicuous object before sunrise.

Saturn, the ringed planet, is still farther off, and his orbit is so large that only a small portion of it can be shown on the map. Although he is on the west side of the line C D, and therefore morning star on the 1st of the month, yet he rises so early in the evening that he is very conspicuous during the whole month. On the 5th of the month Saturn is in opposition to the sun and becomes evening star. On that day he is nearest to the earth, being then 755,420,000 miles distant from us, and as the earth's motion is away from Saturn, by the end of the month he is nearly eight million miles further.

No one looking at this map can avoid seeing the influence that proximity to the sun has upon the motion of the planets. During the month Mercury sweeps more than one-third of his course around the sun, while Saturn makes scarcely $\frac{1}{112}$ part of his circle, the other planets moving with intermediary velocities. It is not only that Saturn's orbit is so much larger, but his motion is so very much slower. In every second Mercury sweeps over twenty-nine miles of space, while Saturn in the same time moves less than six. The greater the distance the planets have to go, the more slowly they go about it. In the case of Mercury and Saturn, it is like two travelers starting from New York. One has to go to Philadelphia, and he takes an express train. The other, going to San Francisco, mounts an ox cart and takes half a lifetime for his journey.

The orbits of Uranus and Neptune are so large that neither of these planets can be shown on the map. This, however, is of less consequence, as neither of them can be seen by the naked eye. But their directions from the sun are shown on the map by arrows, and their positions may be marked by pins on a larger sheet of paper by measurement on the scale, Uranus at 1,700 millions of miles from the sun, Neptune at 2,755 millions. Otherwise, the cross on the arrow showing the direction of Uranus is just half way to where a pin should be set for the planet, and the cross on Neptune's arrow is just one-tenth of his proportionate distance. Their motions during the month are very small.

The position of each planet among the stars may be ascertained from the map, by drawing a line from the earth through the planet to the edge of the map and noticing the constellation there marked. This is the *constellation* in the heavens in which the planet appears, not the *sign* given in the almanacs, which has no real existence, and is nothing but an astrological fiction. Thus, a line from the earth through Venus shows that planet to be in the constellation Pisces, and one through Jupiter indicates Sagittarius as his place among the stars.

In order to use the map as an index to ascertain the positions of the planets at any time, it is only necessary to hold the map in such a position that a line drawn from the earth through any known object shall correspond with the direction of that object; all the planets and the sun itself will then lie in the directions shown by the map. For instance, if at three o'clock in the afternoon of February 1, the map be so held that a line drawn upon it from the earth through the sun points directly to the sun, the position in the sky at that moment of the planet Venus is shown by a line drawn from the earth through Venus, and so of all the other planets. Or, if at eight o'clock of the evening of the same day we draw a line upon the map from the earth through Venus, and so hold the map that the line will point to that planet, the positions of every other planet and the sun will correspond with the directions to which lines drawn from the earth to each planet point in the sky at that moment.

THE MOON.

Few of our readers will be surprised that the path of the moon is not drawn on the map. The full line representing the path of the earth, narrow as it is, is wide enough to completely obliterate any line that might be drawn to represent the actual path in space of the moon as it circles round the earth. The map would have to be enlarged to a width of about twelve feet in order to show the moon at a distance from the earth of one sixteenth of an inch, and on such a map the earth's orbit would be drawn with a radius of 23¼ inches, the scale being four million miles to the inch. Even then, the path of the moon being so nearly coincident with that of the earth, the line of the moon's path would make no impression on the eye as being circular to the earth as its center. But viewed from the earth itself, it is by far the most conspicuous object in the heavens in the absence of the sun, and by its rapid motion it appears in conjunction with every planet during the month.

It began its career in 1889 by causing a total eclipse of the sun on the 1st of January, and while January had two new moons, the present month, having only 28 days, has none. The moon is in conjunction with Mars on the 2d and with Venus on the 3d and again with Neptune on the 8th. Its next conjunction will be with Saturn at 7.30 o'clock on the evening of the 14th. Being then nearly full and quite near the planet, the conjunction will be quite interesting. The conjunctions with Uranus on the 19th, with Jupiter on the 24th, and Mercury on the 27th will not be specially interesting.

The month of February is one of the seasons of the year 1889 during which the moon varies in its distance from the earth less than usual. It is farthest from the earth at five o'clock in the afternoon of the 9th, at which time it is 251,420 miles from us. It is nearest to the earth at about the same hour on the 24th, when it will be 229,720 miles distant.

NOTES ON METEORITES.

IDENTITY OF ORIGIN OF METEORITES, LUMINOUS METEORS, AND FALLING STARS.

IT is very fortunate for science that many of the meteorites so carefully preserved in our museums *have been seen to fall*. This being so, we possess full accounts of the accompanying phenomena and effects.

These comprise the most vivid luminosity; visible and audible explosions, in some cases heard over thousands of square miles of country; and at times a long train in the sky indicating the meteor path, which sometimes remains visible for hours.

Now, precisely similar effects have been noted when nothing has reached the earth's surface; and in the thousands of records of the phenomena presented by luminous meteors, fire balls, bolides, or shooting or falling stars, as they have been variously called, we have the links which connect in the most complete manner the falls of actual irons and stones from heaven with the tiniest trail of a shooting or falling star, *une étoile qui file, qui file, et disparait.*

The heavy masses fall by virtue of their substance resisting the friction of the air; the grains are at once burnt up, and fill the upper regions of the earth's atmosphere with meteoric dust.

As we have seen, the weights of meteorites which have actually fallen vary between many tons and a few ounces, the latter being, in all probability, fragments shattered by the explosion. In case of some shooting stars, the actual weight involved has been estimated by Prof. Herschel as low as *two grains*, not one out of twenty estimated by him exceeding a pound.

It may appear impossible that such atoms should produce the brilliant effects observed, but Prof. Herschel has calculated that a single grain moving at the rate of 30 miles a second represents a dynamical energy of 55,675 foot pounds. This energy is converted by the resistance of our grosser air into heat, as the motion of a projectile is converted into heat by its impact on the target; * and hence the combustion of the matter of the meteorite, and perhaps even the incandescence of the air through which it rushes with such lightning velocity. This luminosity commences often at a height of 80 miles, and sometimes even higher, in regions where the atmosphere must be excessively rare.

Could these little bodies pierce our envelope as readily as do their larger cousins, the meteorite stones and meteorite irons, we should certainly have the advantage of placing them in our museums; but, on the other hand, the bombardment—the *feu de ciel*—might be one to which the *feu d'enfer* of all terrestrial artillery would be, in the gross total of results, as mere child's play.

But the identity of such phenomena as these is by no means the only line of evidence demonstrating the connection now in question.

PROOF FROM THE CHEMISTRY OF FIRE BALLS.

The spectral appearances observed with meteors, fire balls, and shooting stars, which explode and produce luminous effects, are entirely in harmony with those observations on the spectra of meteorites to which I have referred.

The observations, so far as they have gone, have given decided indications of magnesium, sodium, lithium, potassium, and of the carbon flutings seen in comets.

Prof. Herschel and Herr Konkoly have both noticed that in the generality of cases the lines of magnesium (one of the constituents of the olivine) show themselves first in the ordinary meteor or falling star, and the beautiful green light which is so often associated with these falling bodies is due to the incandescence of the vapor of magnesium.

The following quotations from Konkoly and Prof. Herschel are among the authorities which may be cited for the above statement:

"On August 12, 13, and 14, I observed a number of meteors with the spectroscope; among others, on the 12th, a yellow fire ball with a fine train, which came directly from the Perseid radiant. In the head of this meteor the lines of lithium were clearly seen by the side of the sodium line. On August 13, at 10h. 46m. 10s., I observed in the northeast a magnificent fire ball of emerald green color, as bright as Jupiter, with a very slow motion. The nucleus at the first moment only showed a very bright continuous spectrum with the sodium line; but a second after I perceived a magnesium line, and I think I am not mistaken in saying those of copper also. Besides that, the spectrum showed two very faint red lines." †

"A few of the green 'Leonid' streaks were noticed in November (1886) to be, to all appearances, monochromatic, or quite undispersed by vision through the refracting prisms; from which we may at least very probably infer (by later discoveries with the meteor-spectroscope) that the prominent green line of magnesium forms the principal constituent element of their greenish light." ‡

Again, later on in the same letter, Prof. Herschel mentions Konkoly's observation of the bright *b* line of magnesium, in addition to the yellow sodium line, in a meteor on July 26, 1873. I again quote from Prof. Herschel:

"On the morning of October 13 in the same year, Herr Von Konkoly again observed with Browning's meteor-spectroscope the long-enduring streak of a large fire ball which was visible to the northeast of O'Gyalla. It exhibited the yellow sodium line and the green line of magnesium very finely, besides other spectral lines in the red and green. Examining these latter lines closely with a star spectroscope attached to an equatorial telescope, Herr Von Konkoly succeeded in identifying them by direct comparison with the lines in an electric Geissler tube of marsh gas. They were visible in the star spectroscope for eleven minutes, after which the sodium and magnesium lines still continued to be very brightly observable through the meteor-spectroscope." §

Another series of observations | gives continuous spectra for the nucleus, and two trains with sodium, and a third with sodium and a predominant green band, which was doubtless *b* of magnesium, the meteor itself being of emerald green color.

In cases where the temperature has been higher, the bright line spectrum of iron has been associated with the bright lines of magnesium in the spectrum of the falling star, so that the two substances which are among the chief constituents of stones and irons—precisely the two substances which we should expect to find—are actually those which have been observed.

The two lines which Konkoly supposes are probably due to copper will, I expect, be found to be iron lines when other observations are made of the spectra of meteors.

These spectral appearances are naturally associated with colors, and again we find that the colors of the trail, when meteorites have fallen, closely resemble those observed when no fall has been observed.

Green is a tolerably common color, especially in slow-moving fire balls about equal to Venus in luster. These generally leave a short trail of red sparks.

About 10 per cent. of all shooting stars show a distinct color, the most usual being orange or red, the slowly moving ones generally being red. The larger ones, or those with the longest trails, often turn from orange to bluish white, like burning magnesium. Sometimes the change is very sudden and startling.*

A purple or mauve tint, like that given by copper, is sometimes seen.

PROOF FROM THE AURORA.

When we come to consider the number of meteorites which fall upon earth daily, we shall find that it is enormous; and this being so, if we can trace this dust in the air, or after it has fallen, or both if chemical examination shows it to be identical with that of meteorites, we shall be supplied with another argument which can be used in support of the fact that the bodies which produce the dust are meteoric in their origin.

One must suppose that these meteors in their passage through the air break into numerous fragments, that incandescent particles of their constituents, including nickel, iron, manganese, and the various silicates of iron, are thrown off, and that these or the products of their combustion eventually fall to the surface as almost impalpable dust, among which must be magnetic oxide of iron more or less completely fused. The luminous trains of falling stars are probably due to the combustion of these innumerable particles, resembling the sparks which fly from a ribbon of iron burnt in oxygen, or the particles of the same metal thrown off when striking a flint. It is known that such particles in burning take a spherical form, and are surrounded by a layer of black magnetic oxide.

How are we to trace this dust in the air? It is well known that at times the air is electrically illuminated, not only by the flashes of lightning which pass along its lower levels, but by so-called "auroral" displays in its higher reaches.

It is now many years since the idea was first thrown out that the aurora was in some way connected with shooting stars.

M. Zenger has prepared a catalogue of auroræ observed from 1800 to 1877, in which he shows an apparent connection between the brightest displays and the appearance of large numbers of shooting stars.

M. Denza noted the same connection on November 27, 1872, and remarked that he had noticed it before.

Admiral Wrangel, as quoted by Humboldt, observed that in the auroras so constantly seen on the Siberian coast, the passage of a meteor never failed to extend the luminosity to parts of the sky previously dark.†

It is clear that in such a case as this the spectroscope is the only chemical aid applicable, and it has long been recognized that the spectrum observed is *not* the spectrum of the constituents of the atmosphere, as we can study it in our laboratories.

The spectrum, however, strictly resembles that seen in the "glows," to which reference has been made; if the factors present in both cases are meteor dust, low pressure, and feeble electric currents, the resulting phenomena should not be dissimilar.

The results of recent inquiries certainly justify us, therefore, in concluding that the upper reaches of the atmosphere contain particles giving us the spectra of magnesium, manganese, iron, and carbon.

The natural origin is the dust of those bodies which are continually entering those regions, and hence the proof afforded by the spectroscopic observation of shooting stars, that they are identical in chemical composition with meteorites, is strengthened by these auroral observations, while, on the other hand, the origin of the auroral spectrum is placed beyond all doubt.

PROOF FROM THE FALLEN DUST.

It is universally recognized that the atmosphere holds in suspension an immense number of very minute particles of organic and inorganic origin. These must be either dust taken up by aerial currents from the ground—the result of volcanic action—or extra-terrestrial bodies. Many scientific men, among whom we may mention Ehrenberg, Daubree, Reichenbach, Nordenskjold, Tissandier, Murray, and Renard, have studied this problem. Dust collected in various places at different times has been examined with a view of determining whether its origin was meteoric. In many cases, in which chiefly definite iron chondroi have been observed, the evidence has seemed very strong in favor of the view.

It is at once obvious that the detection of such dust which falls on the general surface of the land is hopeless, and that that which is collected on snow in inhabited countries containing foundries and the like is doubtful.

But a considerable advance of this question has recently been made in studying the deep sea deposits collected by the Challenger expedition. Messrs. Murray and Renard,‡ in giving the results of their researches, point out that at the greatest depths of the ocean furthest from land, the sea bottom is very different from that nearer the coast lines.

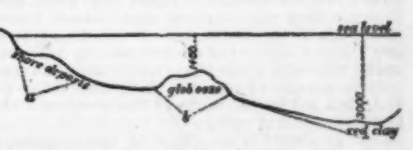


FIG. 1.—Section of ocean showing red clays at depths of 3,000 fathoms (18,000 feet).

* The particles of iron in a large projectile, after impact, which is accompanied by a flash of light, are usually brought to a dark blue color, which would correspond to about 556° F., but the momentary heat imparted is certainly greater than this.

† Konkoly, *Observatory*, vol. iii., 157.

‡ Herschel, letter to *Nature*, vol. xxiv., p. 507.

§ *Ibid.* See also *Astr. Nach.*, No. 2914.

| *Monthly Notices*, vol. xxxiii., p. 575.

* Corder, *Monthly Notices*, vol. xl., p. 83.

† "Cosmos" (Otte), vol i., p. 114.

‡ "On the Microscopic Characters of Volcanic Ashes and Cosmic Dust and their Distribution in Deep Sea Deposits," Proc. R. S. E., and *Nature*, vol. xxix., p. 565.

Under these necessary conditions of exceeding slow deposition and absence from ordinary sources of contamination, it is clear that the problem can be attacked under the best conditions.

We read: "The considerable distance from land at which we find cosmic particles in greatest abundance in deep sea deposits eliminates at once objections which might be raised with respect to metallic particles found in the neighborhood of inhabited countries. On the other hand, the form and character of the spherules of extra-terrestrial origin are essentially different from those collected near manufacturing centers. These magnetic spherules have never elongated necks or a cracked surface like those derived from furnaces, with which we have carefully compared them. Neither are the magnetic spherules with a metallic center comparable either in their form or structure to those particles of native iron which have been described in the eruptive rocks, especially in the basaltic rocks of the north of Ireland, of Iceland," etc.

Messrs. Murray and Renard then state on what they rely in support of their view that many of the particles thus obtained from great depths are of cosmic origin:

"If we plunge a magnet into an oceanic deposit, especially a red clay from the central parts of the Pacific, we extract particles, some of which are magnetite from volcanic rocks, and to which vitreous matters are often attached; others again are quite isolated, and differ in most of their properties from the former. The latter are generally round, measuring hardly 0.2 mm., generally they are smaller, their surface is quite covered with a brilliant black coating, having all the properties of magnetic oxide of iron; often there may be noticed upon them cup-like depressions clearly marked. If we break down these spherules in an agate mortar, the brilliant black coating easily falls away, and reveals white or gray metallic malleable nuclei which may be beaten out by the pestle into thin lamellæ. This metallic center, when treated with an acidulated solution of sulphate of copper, immediately assumes a coppery coat, thus showing that it consists of native iron.

"But there are some malleable metallic nuclei extracted from the spherules which do not give this reaction, they do not take the copper coating. Chemical

FIG. 2.

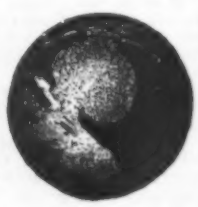


FIG. 3.

FIG. 2.—Black spherule with metallic nucleus (60 : 1). This spherule, covered with a coating of black shining magnetite, represents the most frequent shape. The depression here shown is often found at the surface of these spherules. From 3,375 fathoms, South Pacific.

FIG. 3.—Black spherule with metallic nucleus (60 : 1). The black external coating of magnetic oxide has been broken away to show the metallic center, represented by the clear part at the center. From 3,150 fathoms, Atlantic.

reaction shows that they contain cobalt and nickel; very probably they constitute an alloy of iron and these two metals, such as is often found in meteorites, and whose presence in large quantities hinders the production of the coppery coating on the iron. G. Rose has shown that this coating of black oxide of iron is found on the periphery of meteorites of native iron, and its presence is readily understood when we admit their cosmic origin. Indeed these meteoric particles of native iron, in their transit through the air, must undergo combustion, and, like small portions of iron from a smith's anvil, be transformed either entirely or at the surface only into magnetic oxide, and in this latter case the nucleus is protected from further oxidation by the coating which thus covers it."

We are next shown that these metallic chondroi occur with stony chondroi, so that if the interpretation of a cosmic origin for the magnetic spherules with a metallic center be not considered established in a manner absolutely beyond question, it almost becomes so when we take into account their association with the silicate spherules, never found in rocks of a terrestrial origin. These are thus described:

"Among the fragments attracted by the magnet in deep sea deposits we distinguish granules slightly larger than the spherules with the shining black coating above described. These are yellowish-brown, with a bronze-like luster, and under the microscope it is noticed that the surface, instead of being quite smooth, is grooved by thin lamellæ. In size they never exceed a millimeter, generally they are about 0·5 millimeter in diameter; they are never perfect spheres, as in the case of the black spherules with a metallic center; and sometimes a depression more or less marked is to be observed in the periphery. When examined by the microscope, we observe that the lamellæ which compose them are applied the one against the other, and have a radial eccentric disposition. It is the leafy radial structure (*radialblättrig*), like that of the *chondres* of bronzite, which predominates in our preparations. We have observed much less rarely the serial structure of the *chondres* with olivine, and indeed there is some doubt about the indications of this last type of structure. Fig. 4 shows the characters and texture of one of these spherules magnified twenty-five diameters."

It is worthy of remark that, associated with these chondroi in the red muds at the greatest depths in the ocean, are found manganese nodules in enormous numbers. If a section be made of one of these, a number of concentric layers will be observed arranged around a central nucleus—the same as in a urinary calculus. When the peroxide of manganese is removed by strong hydrochloric acid, there remains a clayey skeleton which still more strongly resembles a urinary calculus, according to Mr. Murray.

This skeleton contains crystals of olivine, quartz, augite, magnetite, or any other materials which were contained in the clay from which the nodule was taken. In the process of its deposition around a nucleus, the peroxide of manganese has inclosed and incorporated in the nodule the clay and crystals and other materials in which the nucleus was embedded. The clayey skeleton thus varies with the clay or ooze in which it was formed. Those from a fine clay usually adhere well together; those from a globigerina ooze have an areolar appearance; those from a clay with many fine sandy particles usually fall to pieces. Mr. Murray attributes

FIG. 4.—Chondros. Spherule of bronzite (25 : 1) from 3,500 fathoms in the Central South Pacific, showing many of the peculiarities belonging to *chondres* of bronzite or enstatite.

the origin of these nodules entirely to the decomposition of volcanic rocks:

"Wherever we have pumice containing much magnetite, olivine, augite, or hornblende, and these apparently undergoing decomposition and alteration, or where we have evidence of great showers of volcanic ash, there we find the manganese in greatest abundance. This correspondence between the distribution of the manganese and volcanic *debris* appears to me very significant of the origin of the former. I regard the manganese, as we find it, as one of the secondary products arising from the decomposition of volcanic minerals.

"Manganese is as frequent as iron in lavas, being usually associated with it, though in very much smaller amount. In magnetite and in some varieties of augite and hornblende the protoxide of iron is at times partially replaced by that of manganese.

"In the manganese of these minerals and in the carbonic acid and oxygen of ocean waters we have the requisite conditions for the decomposition of the minerals, the solution of the manganese, and its subsequent deposition as a peroxide."*

These nodules have been examined in the same way as the meteoric dust. Naturally the chief manganese fluting (the chief auroral line) has been seen.

The question arises, therefore, whether the origin of these deep sea concretionary deposits of iron and manganese, which are unrepresented in any deep sea geological deposit, may not be in part, even if in small part, meteoritic, and represent, like the chondroi, another form of fallen dust.

PROOF FROM SIMILAR VELOCITIES.

Again, the meteorites, as we have seen, enter our atmosphere with very different velocities. The same thing happens with falling stars, which on this account have been divided into three classes, as follows:

Class I. Swift, streak-leaving meteors.
II. Slow, with trains of sparks.
III. Small, quick, short-pathed, sometimes with streaks.

It has also been determined that the luminous effect which is common to the fall of a meteorite or the appearance of a shooting star begins at about the same height. In fact, we have in meteorites, large fire balls, and shooting stars, a progression both with regard to the height at which they become visible and the nearness to the earth at which their luminosity is extinguished.

The actual determination of these heights was commenced by two Göttingen students—Brandes and Benzenberg—in 1798, at the suggestion of Chladni, with the result that the upper reaches of the earth's atmosphere were found to be pierced by bodies entering it with planetary velocities.

Profs. Herschel and Newton were the first to discuss the data accumulated on this subject,† while, as early as 1864, Father Secchi made use of the electric telegraph in securing simultaneous observations.‡ The results of these combined inquiries may be thus shown in the case of shooting stars:

	Beginning. Height in miles.	End. Height in miles.	Authority.
Europe and America, 1798–1863	70·1	54·2	H.
	73·5	50·6	N.
Italy, 1864	74·6	49·7	S.
Average	72·7	51·5	

In Herschel's values fire balls are excluded, and hence the limits are narrower.§ Fire balls often arrive within twenty miles of the earth's surface, and then the concussion is of nearly the same intensity whether stones fall or not.

Such determinations as these, when the observations can be depended upon, can be made with the greatest nicety and by graphical methods. One of the earliest employed—a description of which will give a fair idea of the investigation—is due to Colonel Laussedat.‖

The observations stating the path of each meteor among the stars having been obtained, a 12-inch celestial globe is "rectified" in the usual manner for the place and time. In this way we get first the azimuth and altitude of the beginning and end of each trail. This is done for each place at which the same meteor is observed.

The results are then plotted on a large-scale map, on which the altitudes and longitudes of the places of observation and the distances between them can be determined. The scale of the map permits the height of the intersection of the lines of sight to be at once found, and the agreement or disagreement of the observations can be noticed, thus allowing the worst observations to be rejected.

By taking such observations as these in different places it is possible not only to determine the height at which they enter, but the velocity with which they pass through the upper regions of the air, even supposing they do not eventually get to the bottom. The lowest velocity determined up to the present time is something like two miles per second; the maximum is something like 50 miles a second; but we may say that the average rate of movement is 30 miles a second, which is about 150 times faster than a shell leaving one of our most powerful guns. J. NORMAN LOCKYER.

* Murray, *Nature*, vol. xv., p. 340.

† Herschel, B. A. Report, 1863, p. 328; Newton, *Silliman's Journal*, 2d series, vol. xxxvii., July, 1864.

‡ *Bull. Meteor.*, vol. iii., p. 67.

§ *Monthly Notices*, vol. xxv., p. 159.

‖ *Comptes Rendus*, vol. lviii., p. 1,100, 1864.

ANIMAL ARITHMETIC.

By Madame CLEMENCE ROYER.

ALL degrees of arithmetical aptitude may be found among the human races, from the genius of a Newton and a Laplace to an absolute inability to conceive the abstract notion of number aside from concrete facts furnished by direct perception. The savage is not deficient in the perception of the multiple. He never confounds one tree with two, or two with three, or four; and is well aware of the difference between two, three, or ten men, when he is going out to fight them. The thing that he cannot do is to abstract the idea of a number from the things to which it is attached, and generalize it, without reference to the concrete objects with which he has seen it associated. He may comprehend two, because it is associated with his two hands and his two feet; three, by the aid of the triads with which he is acquainted, and of the triangle with which those objects may be arranged; four, from the four limbs of animals and the four corners of a square. But his ability to form such conceptions is very limited. The first steps in learning in this direction, in savages and children, are to distinguish the abstract notions of unity and plurality, and in plurality, of two and three from larger pluralities. The difficulty in the way of their reaching a concept of abstract numbers is their inability to form a mental representation; four trees not being identical in the savage's thought with four stones, he cannot imagine that there is anything common between them. Nevertheless, he can distinguish clearly enough between four trees and three others, and the two groups will leave quite different impressions in his mind. Four trees in a row will also make a different impression from four trees in a square. He is most struck with differences of distribution in space, and derives from them his notions of differences in plurality. While he is a poor arithmetician, he is a good geometrician.

It is by the exercise of this faculty that he finds his way so readily where he has once gone. He recognizes a wood he has been in by the relative distances apart of the trees, their heights, sizes, the inclination of their trunks to one another, the profile of their masses, and their kinds. He learns the landscape by the relief and accidents of the ground, the wave lines of the horizon, and a thousand details which he fixes upon his memory by a single keen observation so clearly as to give imagination no chance to play tricks with him. He estimates distances by the weakening of tones and the convergence of the lines and planes of the perspective. But in all his comparisons of present and past sensations, definite notions of number rarely have any part.

The inequalities in the development of the power of counting and of arithmetical notions among the different human races should make us cautious about accepting the accounts of those who believe that some animals can be taught to count. It is extremely improbable, at least, that animals, however intelligent, without language, should acquire precise ideas which human beings succeed in apprehending only by the aid of language and education.

The animal can distinguish relative sizes, but the measure of quantities escapes him. Like the child and savage of inferior development, he can only distinguish between few and many, between unity, duality, and plurality, the various degrees of which must inevitably be more or less confused in his perceptions. He knows facts, but he distinguishes them chiefly by their order of coexistence in space rather than of succession in time; and they are photographed in his brain in views of the whole, of which, not being able to separate them from the others, he cannot distinguish the similar parts enough to count them. His recollection of places is a succession of kaleidoscopic pictures, in which, among the moving forms and successive sounds, he marks only what moves him, what gives him pleasure or pain, what flatters his instincts or answers to his wants, what arouses fear or desire in him. If he should go so far as to count, it would be only those objects which interest him with a view to his security.

The wolf and the fox, for instance, can distinguish whether a flock is guarded by one or two dogs, but cannot tell any better than we can how many sheep there are in the flock; but if they see one or two separated from the rest, they will attack them. In the same way we can distinguish the forms of two, three, or four poplars on the bank of a stream, while we cannot tell the number of trees in an avenue, but are always inclined to exaggerate it.

It is by this observation of lines, directions, and signs in the whole, rather than of numbers, that the animal acquires the faculty of recognizing roads it has passed over, and places where it has met its prey or escaped its enemies. It can orient itself to the horizon and measure distances, as the savage does; and, like him, it has the sense of the direction it ought to take to reach the object it is in search of; but it is reasonably certain that no calculation of any number of units enters into this intelligence. When a dog in hunting crosses a wood or a fallow ground, he is able, by a quick apperception, to describe all the curves and all the angles that permit him to avoid or turn the obstacles. He can adjust his leap to the width of the ditch which he has to jump, and find the point where he can pass a live hedge. This mechanical instinct is exercised spontaneously in him, by virtue of exact theorems of which he has no analytical consciousness, but which he applies with a precision that defies the ingenuity of mathematicians. Force, speed, mass, space, and time are calculated in an equation in which the means are closely adapted to the end, with a mar-

velous economy of efforts, and by the quickest if not always the shortest way.

In his relations with man, attentive to the will of his master, whose language he has learned to understand, partially at least, the dog comprehends whether he must go or come, run or stop. But he attends less to the phonetics of the words than to the intonations of the voice, the direction of the glance, and the gestures.

The horse knows whether he is to go right or left by the energetically pronounced interjections "gee" and "haw," but he measures the extent to which he must veer in either direction by the pull on the bridle, so much as to turn quite round if the pull is at once energetic and quick. If the stress is equal upon both sides, he stops abruptly on his haunches. He learns the language of the stable in a very short time, much less than it takes to train the horseman. But there is no notion of numbers in it, although the horse is very sensitive to musical rhythm.

We cannot, at a glance, estimate the number of men in a regiment, but we can calculate it rapidly, when we have noticed the number in a line, the number of lines in a company, and the number of companies. We are still more incapable of distinguishing one hundred points in rows from ninety or one hundred and twenty, without dividing them into tens. But an animal, which has no faculty of numerical abstraction, or of unities of different orders, or of multiplication, would be absolutely incapable of performing this operation. When small numbers of concrete things are in question, however, the aptitude of animals to distinguish these numbers is evident, and is, moreover, indispensable to their existence. A wolf or a boar, which could not perceive how many dogs were attacking it, would not know how to defend itself; but, if it counts them, it is by means of the tactics by which it opposes them, and in which it follows geometrical rules; for if four dogs attack it, two on the right and two on the left, it retreats, facing them, so as not to be between them, but to hold them as much as possible in front and within reach of its tusks. But if the pack is too numerous, the animal becomes wild, loses a clear notion of the number of its adversaries, and bites at hazard the one which presses closest upon it. A man in a similar situation would do much the same.

Birds have at least a vague idea of the number of eggs in their nests, for we cannot take one out without causing in them a disquiet that becomes greater if we remove more. But they manifest a like distress when their eggs are only disarranged. Is this because the geometrical arrangement of the eggs is changed? Five eggs or four make a symmetrical arrangement as the bird disposes of them. But if some are taken out, and three, or two, or one are left, the disposition is very perceptibly changed. When the little birds have been hatched, the differences in their size, liveliness, figure, and voice give the mother a means of distinguishing them individually. And even the eggs are perhaps not so indistinguishable to her as to us; for sexual and maternal instinct conveys special faculties in these matters. The domestic fowl seems to be less intelligent in such things than sparrows and other wild birds, but this is because domestication has modified her instincts.

Cats certainly know how many kittens they have, but they seem less affected by the loss of one of them, according as more are left. If the mother loses one of four or five, she seeks for it a little while with considerable anxiety, and then becomes reconciled to the loss. If only one is left, she becomes greatly troubled, and, if that is taken from her, her distress appears extreme. This may be because of the pain she suffers in her teats when the milk ceases to be removed. When the young have become weaned, she can witness their disappearance with apparent indifference.

Dogs have been observed on various occasions to exhibit primary numerical perceptions in the concrete. Where there are a number of them in a house, they quickly remark the absence of one of their companions. But they are rarely troubled by it, and make no effort to find the missing one, and they are still more ready to take notice of the absence of one of the members of their master's family. These traits are more easily explained by the clear knowledge which dogs have of individualities than by ascribing to them notions of unities as forming parts of numbers. The unequal degrees of attachment which they show for the different members of the family, and for the different persons who live in the house or visit it, proves that they make great differences between them. The idea of difference between several persons involves and supports a notion of their number. But it may lie there if wrapped up in their total perception.

Hounds pursuing a hare are troubled for an instant if they raise another one, and will sometimes stop, as if they were uncertain which one they ought to follow. Good dogs will not allow themselves to be diverted from the scent of the first animal, which they have already tired. When the setter pauses before a flock of partridges, the movements of his head and eyes follow the birds that stray to the right or left. If the flock is large, he cannot estimate their number, nor can the hunter. The shepherd's dog watches his flock of sheep, and goes after the individual members that wander from it. But if one or two of them have been sold or carried off by a wolf during his absence, he does not miss them.

It is wholly improbable that the so-called learned dogs, which are said to have been taught to count, have really been lifted to the abstract notion of numbers. They have simply been taught to associate certain signs or words of their master with particular graphic signs, the geometrical figure of which has been impressed upon their memory. In the same way that the horse associates the words "gee!" and "haw!" with his right and his left, and dogs associate the sound of the horn or trumpet with the chase, they may associate with figures which are shown to them the names as pronounced of those figures, but without comprehension of their numerical relations, and without distinguishing them in any other way than by the difference in their shape. They may be taught to arrange them when commanded in any particular order, without it being necessary to suppose that they have any idea of their arithmetical significance.

When Sir John Lubbock speculated about teaching his dog to read, he played upon this faculty of associating vocal signs with certain forms or figures, and even with graphic signs, figures, or letters. The dog certainly had no comprehension of the ideographic value of the figures which were shown him drawn upon the pasteboard; but their shape, stamped in his memory, was associated with the sounds that were spoken to him when they were shown him, and with certain acts that he was to perform to obtain caresses and rewards from his master. He thus soon learned to pick out the cards which he had to bring to ask for drink or food or to go out in order to have his desire satisfied. The quickest way of teaching children to read is to show them at the same time the image of the object and the word that designates it, so that the two shall be associated in their minds, and they are tempted to speak in the same way. But to appreciate the abstract sense of the noun or the verb requires a degree of intelligence and faculties of comparison which none of our domestic animals has as yet attained.

It would, moreover, be very extraordinary to find in an animal so far removed in organization from man as the dog a *quasi*-identity of mental faculties, and an educability which is wanting in entire human races. It is enough to show that between the intellectual state of dogs and of Bushmen, Tasmanians, and Veddahs, who can count only two, and then say "many," the difference is as slight as possible, and the passage insensible.

It is, moreover, evident that horses and dogs know as well as a savage that they have four limbs. Foxes caught in traps will use the most ingenious devices to extricate themselves, and will even gnaw off one of their paws rather than be prisoners. This requires an effort of the will contrary to the instincts, surpassing the degree of moral energy of which most men are capable; and, further than this, the act demonstrates a power of applying means to the end, which is an act of intelligence not less complicated than the effort required for counting twenty on the fingers and toes, as is the manner with most savages.

The faculty of abstraction and generalization is developed exclusively by the aid of descriptive and ideological language, which classifies things and acts under different words or auditive images. With inferior races this faculty is weaker in proportion as their language is less analytical and less rich in abstract terms. It is impossible to excite it in an animal, because in the absence of a language common to man and him, he is destitute of every means of acquiring it. There is really no bridge between animal and human intelligence. While our language, being descriptive and objective, associates a sound with each visual image, the language of the animal only expresses emotions and passions. As a rule, it is as untranslatable to us as our language is to them. It is only when we try to paint, describe, relate, and express ideas, that they cannot understand us, for nothing is easier than to cause them to share our emotions, tenderness, anger, or hatred. They understand our mimicry better than we can understand theirs, and by mimicry we can make them understand the causes of our emotions of a certain kind. The only condition is, that we be dealing with species of a social nature.

When a hunting dog sees his master with his gaiters, carrying his game bag and gun, he understands that he is to go with him. He may even have acquired the habit of associating the recollection of a sound with these objects, and thus know the names by which we designate them in the language which he hears us speak. He may also be taught to fetch the gaiters, shoes, and game bag when told to do so. If, when he has brought one shoe, he is told to fetch the other, he understands that there are two. To this point he certainly has the notion of duality. He cannot be ignorant, after he has executed this order several times, that the words "the other" mean the second shoe. If, after having been trained by an English master, he passes over to a Frenchman, he learns that his order *l'autre* means the same thing. He takes no notice of the difference in the sound of the words, because they are both uttered with the same accentuation and intonation, and under the impulse of the same feelings. To him human speech is a yelping, which he interprets by the same rule as he does his own ejaculations.

A second unit added to the first is in reality the beginning of all numeration and the foundation of arithmetic. As there are human races that have never gone further, we need not be surprised if animals stop there. But they do not stop there. They go on from this by successive additions, while we have reached the stage of multiplication, and have framed arbitrary systems of numeration, and have thus made of calculation an art founded on ideal notions. Animals, on the other hand, have a concrete notion of numbers more highly developed than we suppose, and perhaps more highly developed than it is with us, in proportion as abstraction is less easy with them. We need not suppose that the animal is destitute of all abstract notions and incapable of all generalization. Far from it; but the general notions being made of resemblances and the individual notions of differences, it is more struck than we are by individual characteristics. In this it again approaches the child and the savage, neither of whom has the generic notion of man. The child has the individual notion of its mother or its nurse, whom it distinguishes from all other persons around; but the generic notion, composed of all the common traits of the persons around, is of slow growth. The languages of savages are, for the most part, wanting in words for *tree* or *animal*, to comprehend the class, but have definite names for all the trees and animals that are useful to the tribe, or which they fear. We, therefore, may affirm that the dog has no generic idea of man, animal, or plant, but only ideas of particular men, particular women, or children; and that every species, whether of animal or plant, is thought of by it as a representation of its individual figure, with all the differences that distinguish it from the others that it has seen. Our imagination by itself cannot bring up the idea of an animal or plant which is not a particular animal or plant; and any effort we may make in this direction will end in there passing through the mind a succession of images of different animals and plants. If the use of generic names is taken from us, the general notion will go with it.

In the absence of articulate and descriptive language, and there being no object competent to serve us as a phonetic and auditive representation, we would think directly of things by a kind of precise interior view that permits no error or verbal sophism, and not as by a kind of internal audition that tends to replace things by their names, that makes us speak our thought within ourselves before speaking it aloud, and which we mistake, as well as deceive others, when the interior definition which we give to the words does not correspond with the thing defined. It is especially difficult, in the absence of a common language between man and the animal, to make the latter comprehend what we require from it, and the object of the acts which we solicit it to perform. The dog has no desire except to obey and please us; the trouble is in explaining to him what we want of him.

A dog had been taught to go, when commanded, to the shed for wood for the fireplace. The exercise amused him; and, when he had brought one stick, he liked nothing better than to return for another, so that he had to be told to stop. But one time when he was alone and lonesome, he pulled down all the wood, stick by stick. He had not comprehended the purpose of the act which they made him perform, supposing it to be a sport, like the ordinary carrying of a stick. Could this dog have been taught to count by sending him for two sticks and then for three, and so on to larger numbers? We doubt it, because he had not even disengaged from the act which he was ordered to do the general idea that all the pieces of wood which he brought were to be burned in the fireplace, that he was never sent for them except for that purpose, and that he should only fetch as many as were needed.

If efforts to educate animals have been even more fruitless in the hands of scientific investigators than of workingmen proceeding without theoretical views, it is because great errors have been committed in the analysis of human faculties, in making such suppositions, for instance, as that arithmetical notions are more elementary than geometrical ones. Having done this, they have sought to teach animals, whose capability is for measuring, to count. Having become habituated by our industrial civilization and the economical laws of exchange to the intervention of the idea of number in all our wants, acts, and works, we have lost perception of the insignificant part which it has in animal life as compared with that of the idea of size. Animals have a very exact sense of size. They can measure time and distance better than we can. The sparrows in our parks, when affecting the highest degree of confidence in us, know how to keep just enough distance from us to be able to evade us. It also seems to be demonstrated that all animals have more or less of the faculty of estimating the number of objects coexisting in space; that is, in a varying degree, of analyzing the similar or identical elements in their visual or auditive perceptions, so long as the number is small enough.

Have they also the faculty of estimating numbers as successive repetitions of the same facts in time, or of counting the reiteration of the same perceptions? I was once told of a workman who was in the habit of giving sugar every day to a dog which he met in going to his work. The dog counted on his daily return. He gave three pieces of sugar, one after the other, and the dog would wait and look till it had got the third piece, when it seemed satisfied and did not ask for any more. It had, therefore, the notion of these three successive facts, and could count them. I learn from good authority that a tame sparrow was accustomed to go out daily from the house where it lived in freedom just before it was time for the children to come out from school. It would wait at the school house door for a child of the family with which it lived, and return perched upon its shoulder. One day it went out but did not return, having probably fallen a prey to a cat.

Nothing is more frequent among animals than daily acts at fixed hours; but we have proof also that animals can measure longer periods. A dog was used to go every Saturday evening for his master, who came to spend Sunday at home, and went away again on Monday. But the dog, instead of following his master away, showed his displeasure at the parting by sulking in a corner. Could this dog count the six days of the week during which his master was absent? It is more likely that his return was foreshadowed by certain things going on in the house that only occurred on that occasion.

Houzeau de la Haie tells of a pelican living in a fisherman's family at Santo Domingo that was fed upon the refuse of the fish cleaning. Looking for its food, it went to the shore every day and waited for the boats to come back. The fishermen rested on Sunday, and the bird acquired so clear a notion of the return of that day, when it had to fast, that it would not stir from the tree on which it was accustomed to spend its time. It is not necessary to suppose that the pelican had learned to count the six days at the end of which its masters would not go fishing; but, while it really estimated daily the time when it must make its excursion to the shore, it was informed of the return of Sunday by observation of what was going on in the house, as, for instance, by the fishermen putting on their Sunday clothes; in the same way as the dog knew when its master was going to hunt by seeing him with his gun and game bag. In such instances, animals show that they have the faculty of associating ideas, of observing consecutive facts, and establishing a correlative connection between them—things which have been proved by abundance of other evidence, and which demonstrates not less intelligence than acquaintance with the ten signs exposing the first ten numbers, or the use of a system of numeration to express larger numbers.

Broderip tells of an English Protestant minister's dog which escaped every Sunday and followed its master to church. It was shut up on one Saturday evening, but on the next week when they went to shut it up it could not be found, and hid itself till the service hour on Sunday, when it appeared again at the church. In acting thus, it had evidently reasoned out all its conduct, displaying memory, foresight, and calculation. It is not likely, however, that it acted upon a count of the days, but rather on the knowledge that it had to turn the spit on Saturday, or the day before its master went to church.

If this period of the seven days of the week does not exceed the intelligence of a dog, the dog should be able easily to measure periods of two or three days. Houzeau says that he tried for three consecutive weeks to repeat the same walk with his dogs, every two days at exactly the same hour. It would have been enough for them to count two to determine the period. On the twentieth day, or the tenth periodical repetition of the excursion, although the dogs enjoyed the excursion

exceedingly, he never remarked that they anticipated it spontaneously, or had a thought of it before witnessing his preparations to go. From this, Houzeau concluded that dogs could not count the days. But when actions repeated daily at fixed hours were in question, the dogs knew when the time came. Broderip's dog and the Santo Domingo pelican had learned, in the course of years, that the same succession of events took place every Sunday. It was not, therefore, by an isolated fact, but by an aggregation of facts, that they became aware of the return of that day; for not only did certain things take place regularly in the family, but Sunday noises, like the ringing of the bells, and unusual comings and goings, occurred in the place. After continued experience, the animals acquired knowledge of the succession of the events, and governed their conduct accordingly.

Houzeau also learned that some animals are capable of measuring lapses of time that particularly interest them. He says that female crocodiles abandon their eggs in the sand for ten or fifteen days, according to the species, and return to the spot at the exact time when they are to be hatched. It is easily conceivable that animals have, in general, a more precise measure of periods which concern the needs of their organic or specific life, than of the more artificial periods to which they have become habituated in the domesticated state or in consequence of their relations with man, because an hereditary habit has always more force than habits acquired by education.

Houzeau cites facts showing that some animals can count the number of similar objects or acts, provided the numbers are not too high. When a magpie is watched by a company of hunters, it will not move till they go away. If they go one after another, it cannot be deceived by one of them staying behind unless there are more than four of them. Another story of similar bearing is that of the tramway mules at New Orleans, which are relieved and fed after making five trips. They make their trips patiently and quietly till the end of the fifth, when they give evident signs that they expect their usual refreshment. The horses in the coal mines of Hainault make thirty trips a day, taking their places again, after every trip, at the head of the train, in readiness for the next trip; but at the end of the thirtieth trip they turn their heads in the opposite direction, or toward the stable.

Facts of this kind ought to be tested by most precise experiments bearing upon the conditions under which they are produced, and upon different subjects. Are not the horses warned at the end of their stint by some exterior sign, such as a change of conductors, the departure of a squad of workmen, or the arrival of the horses that are to take their places, or by the meal hour? Is not the conclusion that they count the number of their trips arrived at too quickly?

It would predicate a very high degree of development to suppose that a horse could count up to thirty in any given number of hours. A man in such case would nearly always make mistakes, unless he had some means of registering the trips as they were completed.

It is nevertheless established that some birds and quadrupeds are capable of counting up to four or five, and perhaps more. It cannot be disputed that the higher limit of this faculty may vary according to species, and also to individual traits, since the mathematical faculties of men are very great in their variations. But we have reasons for believing that the geometrical faculty in animals supersedes the arithmetical faculty, and that the latter has been developed in man under the influences of industrial civilization and commercial exchanges, which have, in nearly all cases, caused the notion of numbers to be substituted for that of measure. — *Translated for the Popular Science Monthly from the Revue Scientifique.*

[AMERICAN CHEMICAL JOURNAL.]

ON THE OCCURRENCE OF SOLID HYDROCARBONS IN PLANTS.

By HELEN C. DE S. ABBOTT and HENRY TRIMBLE.

WHEN many plants of the higher botanical orders are exhausted with petroleum-ether, crystalline compounds may be separated from the extracts which have not been previously noticed. The compounds are also obtained when alcohol or ether is used as a solvent; but it is preferable, on account of the greater number of constituents extracted by these menstrua, to employ petroleum-ether, and thus avoid certain difficulties of separation. Among the plants in which to this time these compounds have been discovered, may be mentioned *Cascara amarga*, *Phlox Carolina* and the *Phlox* species, and in other species of three or four natural orders.

The crystals from these petroleum-ether extracts were first noticed in 1884 and were announced in 1886 as a "camphor-like body."

Subsequent investigations proved this body to be a mixture; and by extracting 25 and 20 kilos of *Cascara amarga* and *Phlox Carolina* respectively, a satisfactory quantity was obtained. The method adopted to purify was, after removal of fats and coloring matter, to crystallize from boiling absolute alcohol. By fractional crystallization at least three different compounds have been separated. One only of these has been studied. It melted at 196·2° C. to 196·4° C.; at higher temperatures it was decomposed, with an odor like sandal wood. It was soluble in petroleum-ether, ethylic and acetic ethers, benzol, chloroform, hot alcohol, glacial acetic acid, acetic anhydride, and linseed oil. From most of these solvents it readily separated in silky acicular crystals, often two to four centimeters in length.

The first ultimate analysis of this purified product from *Cascara amarga* gave the following results:

	I.	II.	III.
C	86·30	86·29	86·33
H	13·96	13·96	13·83
	99·26	99·25	99·16

These figures pointed strongly to the conclusion that the compound was a solid hydrocarbon. Liquid hydrocarbons are abundant in the plant kingdom, but a similar occurrence of this class of compounds in a solid or crystalline condition appears not to have been noticed.

By further purification of an additional quantity of the mixture the following results were obtained:

I. 0·1058 gramme gave 0·3413 CO_2 and 0·1133 H_2O.
II. 0·1118 gramme gave 0·3588 CO_2 and 0·1193 H_2O.

	I.	II.
C	87·97	87·89
H	11·89	11·90
	99·86	99·79

Fifteen kilos of *Phlox Carolina* were exhausted, the compound separated and repeatedly crystallized. Its ultimate analysis gave the following:

I. 0·1117 gramme gave 0·3600 CO_2 and 0·1208 H_2O.
II. 0·1314 gramme gave 0·4238 CO_2 and 0·1421 H_2O.

	I.	II.	Theory for $(C_{11}H_{12})x$.
C	87·90	87·76	88·00
H	12·03	12·02	12·00
	99·92	99·78	100·00

ELECTRICITY NOT A FLUID, BUT A MODE OF MOTION.

IN a recent paper read before the New York Electrical Society, Dr. P. H. Van der Weyde says: "Heat and light will both pass through a vacuum perfect enough to obstruct absolutely the passage of electricity. If there were such a thing as an electric fluid, it would surely pass through any empty space, and we are therefore driven to the conclusion that the presence of matter is as absolute a condition for the transmission of electricity as the presence of air is an absolute condition for the transmission of sound; and there is as little necessity to accept the hypothesis of the existence of an electric fluid as there is for the hypothesis of a sonorous or caloric fluid."

A New Catalogue of Valuable Papers

Contained in SCIENTIFIC AMERICAN SUPPLEMENT during the past ten years, sent *free of charge* to any address. MUNN & CO., 361 Broadway, New York.

THE SCIENTIFIC AMERICAN Architects and Builders Edition.

$2.50 a Year. Single Copies, 25 cts.

This is a Special Edition of the SCIENTIFIC AMERICAN, issued monthly—on the first day of the month. Each number contains about forty large quarto pages, equal to about two hundred ordinary book pages, forming, practically, a large and splendid **Magazine of Architecture**, richly adorned with *elegant plates in colors* and with fine engravings, illustrating the most interesting examples of modern Architectural Construction and allied subjects.

A special feature is the presentation in each number of a variety of the latest and best plans for private residences, city and country, including those of very moderate cost as well as the more expensive. Drawings in perspective and in color are given, together with full Plans, Specifications, Costs, Bills of Estimate, and Sheets of Details.

No other building paper contains so many plans, details, and specifications regularly presented as the SCIENTIFIC AMERICAN. Hundreds of dwellings have already been erected on the various plans we have issued during the past year, and many others are in process of construction.

Architects, Builders, and Owners will find this work valuable in furnishing fresh and useful suggestions. All who contemplate building or improving homes, or erecting structures of any kind, have before them in this work an almost *endless series of the latest and best examples* from which to make selections, thus saving time and money.

Many other subjects, including Sewerage, Piping, Lighting, Warming, Ventilating, Decorating, Laying out of Grounds, etc., are illustrated. An extensive Compendium of Manufacturers' Announcements is also given, in which the most reliable and approved Building Materials, Goods, Machines, Tools, and Appliances are described and illustrated, with addresses of the makers, etc.

The fullness, richness, cheapness, and convenience of this work have won for it the **Largest Circulation** of any Architectural publication in the world.

MUNN & CO., Publishers,
361 Broadway, New York.

A Catalogue of valuable books on Architecture, Building, Carpentry, Masonry, Heating, Warming, Lighting, Ventilation, and all branches of industry pertaining to the art of Building, is supplied free of charge, sent to any address.

Building Plans and Specifications.

In connection with the publication of the BUILDING EDITION of the SCIENTIFIC AMERICAN, Messrs. Munn & Co. furnish plans and specifications for buildings of every kind, including Churches, Schools, Stores, Dwellings, Carriage Houses, Barns, etc.

In this work they are assisted by able and experienced architects. Full plans, details, and specifications for the various buildings illustrated in this paper can be supplied.

Those who contemplate building, or who wish to alter, improve, extend, or add to existing buildings, whether wings, porches, bay windows, or attic rooms, are invited to communicate with the undersigned. Our work extends to all parts of the country. Estimates, plans, and drawings promptly prepared. Terms moderate. Address

MUNN & CO., 361 BROADWAY, NEW YORK.

THE

Scientific American Supplement.

PUBLISHED WEEKLY.

Terms of Subscription, $5 a year.

Sent by mail, postage prepaid, to subscribers in any part of the United States or Canada. Six dollars a year, sent, prepaid, to any foreign country.

All the back numbers of THE SUPPLEMENT, from the commencement, January 1, 1876, can be had. Price, 10 cents each.

All the back volumes of THE SUPPLEMENT can likewise be supplied. Two volumes are issued yearly. Price of each volume, $2.50 stitched in paper, or $3.50 bound in stiff covers.

COMBINED RATES.—One copy of SCIENTIFIC AMERICAN and one copy of SCIENTIFIC AMERICAN SUPPLEMENT, one year, postpaid, $7.00.

A liberal discount to booksellers, news agents, and canvassers.

MUNN & CO., Publishers,
361 Broadway, New York, N. Y.

TABLE OF CONTENTS.

PAGE

I. ASTRONOMY.—Notes on Meteorites.—By J. NORMAN LOCKYER. The identity of the origin of meteorites.—Numerous meteors and falling stars considered, including the proof from the chemistry of fire balls, from the aurora, from fallen dust, and from the identity of philosophy.—4 illustrations..... 10015
The Planets for February, 1889, by "Royal Hill." An interesting practical article for sky observers, with a diagram showing the relation of the earth to the heavens in the present month.—1 illustration..... 10014

II. BIOLOGY.—Animal Arithmetic.—By MADAME CLEMENCE ROYER.—An interesting review of the development of intelligence in the lower animals, with many graphic illustrations of the appreciation by them of numbers..... 10016
Yeast, its Morphology and Culture.—By A. GORDON SALAMON.—Continuation of these valuable lectures, giving the action of yeast and the yeast plant in the alcoholic fermentation, with tables and illustrations of the different plants.—6 illustrations..... 10010

III. CHEMISTRY.—On the Occurrence of Solid Hydrocarbons in Plants.—By HELEN C. DE S. ABBOTT and HENRY TRIMBLE.—Interesting experiments in plant chemistry, giving the results of extractions upon large quantities of two families of plants..... 10015
Water Supply to Steam Boilers.—By THOMAS T. P. BRUCE WARREN.—An interesting and valuable note on the corrosion of boiler plates by use of condensed steam for feed water, chemistry of the process examined..... 10007

IV. CIVIL ENGINEERING.—La Louviere Hydraulic Canal Lift.—A substitute for locks on Canals, as in practical operation in Belgium, being the largest hydraulic canal lift in the world.—3 illustrations. 10004
The Nicaragua Canal.—By Commander H. C. TAYLOR.—A very full review of work done and of the prospects of this great enterprise, the route to be traveled, preparations for work, and general features of the territory.—2 illustrations..... 10004

V. ELECTRICITY.—Improved Accumulator.—By A. RECKENZAUN.—A new accumulator excited by caustic potash, using electrolytically reduced copper as a positive electrode, with elaborate table of results obtained..... 10013
Phenomena Produced by Electric Discharges on Eastman's Pellicular Paper.—By M. E. L. TROUVELOT.—Interesting application of sensitized paper to the study of the characteristics and differences of the positive and negative static discharges..... 10013

VI. METALLURGY.—Aluminum Alloys.—An English paper upon the Cowles electric furnace, with description of the alloys, etc., produced thereby..... 10007

VII. MISCELLANEOUS.—Civil Service at Tonquin—Examination of the Literati.—The famous examinations of China graphically described and illustrated, with full account of the ceremonial.—4 illustrations..... 10008
Examination of the Literati at Hue.—A continuation of this subject, giving the routine of the examination and system of awarding marks..... 10009
The Invisible Woman.—A very curious illusion of the last century described and illustrated by the reproduction of a contemporaneous print.—1 illustration..... 10007
The Opening of the Pasteur Institute.—A review of the life-work of Pasteur, of the support given him by the state, and of his bearing under the attacks recently made upon him..... 10009

VIII. PHOTOGRAPHY.—Hydroquinone for Developing Bromide Prints—Hydroquinone Solutions for Keeping.—Further practical details, with formulæ upon the use of this popular developer..... 10009
Stripping Negatives.—How to strip films from plates, with full details of decolorizing the bichromatized gelatine.—A very suggestive and interesting paper..... 10006

IX. PHYSICS.—Electricity, Light, and Heat.—By Prof. C. F. BRACKETT.—A very interesting paper, giving very suggestive statements of phenomena of wires, with final reference to the economy of electric production..... 10012

Useful Engineering Books

Manufacturers, Agriculturists, Chemists, Engineers, Mechanics, Builders, men of leisure, and professional men, of all classes, need good books in the line of their respective callings. Our post office department permits the transmission of books through the mails at very small cost. A comprehensive catalogue of useful books by different authors, on more than fifty different subjects, has recently been published, for free circulation, at the office of this paper. Subjects classified with names of author. Persons desiring a copy have only to ask for it, and it will be mailed to them. Address,

MUNN & CO., 361 Broadway, New York.

PATENTS.

In connection with the **Scientific American**, Messrs. MUNN & Co. are solicitors of American and Foreign Patents, have had 42 years' experience, and now have the largest establishment in the world. Patents are obtained on the best terms.

A special notice is made in the **Scientific American** of all inventions patented through this Agency, with the name and residence of the Patentee. By the immense circulation thus given, public attention is directed to the merits of the new patent, and sales or introduction often easily effected.

Any person who has made a new discovery or invention can ascertain, free of charge, whether a patent can probably be obtained, by writing to MUNN & Co.

We also send free our Hand Book about the Patent Laws, Patents, Caveats, Trade Marks, their costs and how procured. Address

MUNN & CO.,
361 Broadway, New York.

Branch Office, 622 and 624 F St., Washington, D. C.

889.

ent.

in any
llars a

om the
Price,

an like-
yearly,
r $3.50

AMERI-
UPPLE-

ts, and

N. Y.

PAGE

TER.
and
y of
tity
.... 10015

est-
the
dus-
.... 10014

NCH
ell-
of
.... 10016

ON.
n of
rith
.... 10010

in
in-
ex-
.... 10018

ECH
n of
bis-
.... 10007

—A
um,
ons.
very
ver-
ral
.... 10004

r.—
rti-
ble
.... 10013

el-
ion
er-
.... 10013

on
ne.
.... 10007

of
de-
il-
.... 10008

ab-
rd-
.... 10009

on-
.... 10007

fe-
his
.... 10009

ade
cal
uil
ag-
.... 10006

Ke-
to-
my
.... 10012

ks

neers,
sional
their
rmits
very
books
t sub-
ation,
with
only
ess,
York.

ican,
and
, and
orld.

aeri-
ency,
y the
on is
es or

nven-
atent
Co.
atent
and

rk.